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Dr CHRISTISON, President, in the Chair.

At the request of the Council, Principal Sir Alex. Grant, Bart., delivered an address "On the Educational System of Prussia."

Mr President and Gentlemen,—If I were addressing almost any other assembly, I should probably begin by saying that the subject of the educational system of Prussia possesses a peculiar interest at the present moment for two reasons—1*st*, Because the wonderful successes of Prussia make one curious to know all the methods which have been applied to bring that nation to its present state; 2*dly*, Because public instruction is just now one of the chief questions of the day for the inhabitants of Great Britain and Ireland.

But in this Society considerations of the temporary and the contingent would be out of place. And therefore, omitting altogether such allusions, I propose to submit some account and estimate of the Prussian educational system merely as a sort of contribution to human natural history.

Probably no human institution is perfect, and yet I think we may see nature working in and by means of human societies towards constant improvement—that is, towards the best. While a large portion of mankind seem content to remain stationary,

without any desire for progress, there have always been progressive races who have respectively devoted themselves to working out different problems of civilisation. Among these is the problem of national education, for the working out of which Prussia has made great, and, as it is generally thought, successful efforts. At all events, she has accumulated so great a mass of experience on the subject, as to make the history of her efforts worthy of being studied.

It is a common, but erroneous, notion to suppose that education in Prussia is the product of the arbitrary will of modern despotic governments—that it was conceived as a whole by some Minister of Instruction, drawn out on the foolscap paper of a bureau, and then issued by the *fiat* of the State to be accepted by the people. Such an account would be as far as possible from historical truth. But some notion of the kind has obtained currency, perhaps partly under the authority of M. Cousin, who visited Prussia in 1831, and made a report on the state of education there for the French Government. His account of the primary educational system was translated by Mrs Austin, and so became tolerably well known in this country. M. Cousin got hold of a scheme for the organisation of education throughout Prussia, which had been drawn up in 1819 by Von Altenstein, then Minister of Instruction. Viewing matters rather superficially, Cousin referred all he saw to this scheme, as if it had been the cause and origin of the school system which he found. But the fact is that Von Altenstein's document was merely what we would call a "draft bill." It was never carried in the Chambers, and never became law, and it had no more influence on education in Prussia than the several abortive bills for education in Scotland have had on our parochial schools. The curious thing is that Prussia, up to the present day, has never had a substantive Educational Act. Several bills have been drawn up, as for instance in 1819, in 1850, and in 1869, but they have always been ultimately rejected. And the Liberals in Germany are looking forward to the actual passing of an educational law, after more than fifty years of unsuccessful attempts at legislation in this department, as one of the first internal results which will be achieved after the conclusion of the present war.

It is true that the administration of public instruction in Prussia is bureaucratic in the extreme; but this is not the same as saying

that the educational system has been created in a bureau. The schools grew up in accordance with the ideas of the people; the character of the schools has been modified from time to time by public opinion; till within the last sixteen years the schools varied according to the difference of the different provinces; in short, the central Government has only gradually and lately got its grasp on that which it found, but did not create.

The *Volksschulen*, or people's schools, in Prussia were in the outset a product of the Reformation. The great characteristic of Prussian popular education is universality of school attendance under legal compulsion. Now, the legal compulsion is of comparatively late introduction. It was only brought in after the sending of children to school had long been recognised as a religious duty incumbent on all, and had thoroughly become a habit of the people. Just as John Knox was the author of the parochial school system of Scotland, so Martin Luther was the author of the universal school attendance of Germany. The custom dates from a circular letter which, in the year 1524, Luther addressed to the burgomasters and councillors of all the towns in Germany. It was a manly, earnest, powerful appeal, painting in strong colours the neglected condition of the children, and urging that schools should be provided for them. Luther pleaded that each child should go to school for at least two hours a day, giving the rest of its time, if absolutely necessary, to work. This letter had a striking and permanent effect. The town councils, the landowners, and the princes of Germany were stirred up to action; new schools were provided, and the old ones improved all over the country, and the people gradually took up the idea and never dropt it, that to send their children to school was a plain Christian duty.

At the beginning of the eighteenth century, in 1716, King Frederick William, issuing certain ordinances for the regulation of schools, assumes the universal attendance of unconfirmed persons; he merely gives his royal sanction to an existing practice. In 1763 an *Allgemeines Landschulreglement*, or general regulation for country schools, was issued, which for the first time defined the age of school attendance, namely, from five to fourteen. Thus the law was merely an expression, a ratification, and a definition of the custom of the people.

I will now mention the way in which the compulsion is carried out. Compulsory school attendance may be of two kinds—either (1) the parent may be obliged to show that the child is taught somewhere; or (2) the child may be compelled to attend a particular school for which it is registered. The second is, of course, the harsher and more bureaucratic method, and it is distinctively called *Schulzwang*, or school compulsion; while the first and milder obligation is *Schulpflichtichkeit*, or school duty. The second method, while leaving less liberty to the parent, is more efficient from the point of view of the State; and as such it was adopted in Prussia in 1857, and is now the law of the kingdom. The police-office of each place makes out a list of children as they arrive at school age —that is, five years old. It registers each child for the school nearest its dwelling-place, and sends the list to the school board, which now becomes responsible for the child not only joining the school, but also regularly attending for the next eight years—that is, up to the time of its confirmation. The master keeps a register of attendances, and in some places it is the custom, after the first school hour, to send round a messenger to inquire after missing children and the reason of their absence. Each case of absence is marked by the master as "excused" or "unexcused." When unexcused absences occur, it becomes the duty of the clergyman, as chairman of the school board, or of some deputed member of the board, to use moral suasion with the parent or guardian, with the view of obtaining greater regularity. If these means fail, the name of the parent or guardian is sent to the police-office, and he is mulcted with a small fine for each unexcused absence, and, in case of non-payment, is sent to gaol. Mr Mark Pattison (from whose admirable report on the primary schools of Germany most of my details for this part of the subject are taken) mentions that in Berlin, in the year 1856, there were 1780 convictions for irregular attendance, being rather more than three per cent. on the whole number of children on the rolls of the schools. This was thought a very large proportion, and was attributed to the growth of pauperism, and consequent demoralisation in a large city. I am sorry that I have not more recent statistics to offer, but the system remains the same, and I think that we can see its general working.

In that same year, 1856, there were 2,943,251 children of school age in all the Prussian provinces. Of these, 2,828,692 were in attendance at elementary schools, public and private. Of the remainder, 114,559, many were in attendance at the lower classes of grammar schools and real schools, which are open to pupils of nine years of age; others were being educated at home; a few were doubtless invalids, or physically or mentally incapacitated; the residue, which must be small, represents the children of itinerating families who manage to escape getting upon any school register. Even if we suppose that 100,000 children escaped school attendance altogether, that would give less than three and a half per cent. on the entire population of school-going age. But the proportion for most of the provinces is nothing like so large. Out of the recruits that joined the Prussian army during the past year, it is true that exactly three and a-half per cent. of the troops had never had any schooling. But the great bulk of the unfavourable returns is made up of recruits from Posen, a Polish province which has been called "the Ireland of Prussia," and from the natives of East Prussia, whose vicinity to the frontier facilitates their evasion of school attendance. From the province of Brandenburg, only one-eighteenth per cent. of the recruits had not attended school.

On the whole, the law of compulsory attendance in Prussia may be said to be perfectly efficacious in producing the result at which it aims, and it appears to be very seldom complained of. Even in the political disturbances of 1848, this law was not put forward as one of the grievances against the Government. The law is thoroughly in harmony with popular custom; and just as in this country it is a matter of course for the well-to-do classes to send their children without any exception to school, so in Germany it is equally a matter of course for the peasant and the labourer to send off his children every morning to the school which the community has provided. Day schools throughout Germany (as in Edinburgh) are the rule for rich and poor alike, and there is an air of equality given by the spectacle of rich children, as well as poor, going off each day to their respective schools.

The *Schulzwang*, or compulsion to attend a particular school, is of course relaxed in favour of the rich. The parent applies for exemption, stating his reasons, and naming the school (generally a

private one) to which his child is to be sent. In some places he has to pay the school fee all the same to the school for which his child was registered. In two parts of Germany there used to be no law of compulsion, namely, in the free towns of Hamburg and Frankfort-on-the-Maine. Frankfort, however, has now become Prussian. It was said that in these places the attendance of children at school was quite as universal as in Prussia itself; and some persons argue that the custom of the people might be relied on everywhere in Germany, and the law dispensed with. But we have already seen that the growing pauperism of places like Berlin tends to invalidate the custom. The law, at all events, helps to keep the custom straight, else it might well be doubted whether the ideas of the sixteenth century as to the duty of school attendance could be kept alive in manufacturing centres, and in very poor neighbourhoods. In the agricultural districts, it is said that the farmers dislike schools because they raise wages; in manufacturing districts, the parents dislike schools because they deprive them of a certain amount of wages which their children might otherwise be earning. In the cotton manufacturing districts of Saxony, the Government has made an equitable compromise between the claims of industry and of school learning, by allowing a system of half-time schools for children employed in the factories. The children under this system appear to be ultimately as well instructed as those under a whole time system. I think that this experiment deserves particular attention. For I believe that children up to nine or ten years' old can learn as much in three hours per diem as they could learn in six hours per diem, and that light industrial tasks for the remainder of the day would rather tend to develope the intelligence of the child. In Prussia the minimum age for children being employed in a factory is twelve, and up to fourteen no child must work more than six hours per diem. Thus plenty of time is still left for attendance at a three hours' school.

We have now to consider the funds by which the elementary schools of Prussia are supported. There are very few endowments available for them. The Government has at its disposal for educational purposes about L.50,000 per annum, derived from sequestrated Church property, and from charitable bequests. But this is almost entirely devoted to higher education. The elementary

schools may be said, in a word, to be supported wholly by contributions from the annual income of the community, in the shape of —1st, school fees; 2d, local rate; 3d, general taxation. The first step towards providing for the maintenance of a *Volksschule* is, that the proper authorities of the *gemeinde*, or commune, register each family as assessed at a certain rate of school fees for any children that may be of school-going age. In this country there appears to be a sort of repugnance to the idea of a graded scale of fees in proportion to the income of parents. But in Prussia this is the first principle of public instruction. Fees are assessed upon families not in relation to the cost of the school, but solely in relation to the circumstances of those who are to pay the fees. Government, however, fixes a maximum and a minimum rate. No child is to pay more than fifteen thalers, or about forty-four shillings per annum; and the lowest rate (from which there would only be exemption in the case of extreme poverty) is one groschen, that is about three halfpence, per week. Between these extremes the assessment takes place.

The next source of revenue for the school consists in the collections made in the parish church during one Sunday in each year. Then there is a small capitation tax on poor and rich alike, and, finally, a rating on property, estimated by a loose valuation.

Grants from the general taxation of the country for elementary schools are only made in cases where the commune can show real inability, on account of the poverty of its inhabitants, to meet the necessary cost. The Government, however, has occasionally allowed grants for increasing schoolmasters' salaries. It is clear, then, that as the fees are almost always extremely low, the burden of maintaining the primary schools falls mainly upon the rate-payers. This principle was introduced by the *Allgemeines Landrecht*, or general code of Prussia of the year 1794, which lays down that "where there are no endowments for the support of the common schools, then the maintenance of the teacher falls upon the collective householders, without distinction of religion. The contributions requisite for this purpose, whether they be paid in money or kind, must be equitably divided among the householders, in the proportion of their property and holdings."

To show the working of this system in a large city, it may be

mentioned that in Berlin (which has about three times the population of Edinburgh) there were some time ago about 55,000 children in the elementary schools, and it was estimated that each of these children, in addition to the school fees, cost the municipality about L.1 sterling per annum,—the total expenditure on this object being about twelve per cent. on the municipal budget.

We have seen how the primary schools in Prussia are filled, and how they are supported; we have now to inquire how they are managed. The *Volksschule* has never forgotten the tradition of its origin, at the time of the Reformation, as an ecclesiastical institution. The immediate and local management of all the schools is practically in the hands of the clergy. The clergyman of the parish is *ex officio* local inspector of the common school. He is chairman of the school board, which consists of representatives of the householders. He has really onerous duties in connection with the school. He is expected to visit it constantly, in some places as often as once a week. He is not merely the inspector of the school in the sense of examiner and critic, but he is responsible for its management and superintendence. He has to prepare the children for confirmation by a religious lesson of at least an hour a day for the two or three months preceding Easter.

The central power is said to regard the clergy as useful in repressing the instinct of self-government in the commune. The clergy are said generally to take a bureaucratic and centralising point of view in the discharge of their functions as school inspectors. But they have a difficult and thankless office. They have to encounter the jealousy of the school board, and often the discontent and mutiny of the schoolmaster, who has, perhaps, the chronic grievance of an inadequate salary, and who, having been professionally prepared in a training college, finds himself controlled by one who has no technical acquaintance with the details of school management.

In the political disturbances of 1848–49 (which were designated as "the schoolmasters' rebellion"), one of the great cries was for the autonomy of schools, that is, for greater freedom from the control of the Church. And this is one of the things which the Prussian Liberals expect from the Educational Bill of the future. They do not seem to ask for a secular system of instruction, but

rather for emancipation from clerical management. The Government depends much on the moral influence of the clergy in promoting regular school attendance among the people, and generally in playing a conciliatory part in relation both to the school board and the master. In many cases the clergy appear to perform these offices in a most Christian and self-denying spirit. But, on the other hand, they appear frequently to fall into a state of apathy and indifference about the schools. Their labours, as school inspectors, are an unremunerated addition to their proper functions, and are such as often, individually, they have no taste for.

The present system is recommended by its cheapness, as under it school inspection costs nothing to the Government. But, on the whole, it can hardly be called successful, and it is probably doomed to alteration. It is not only the clergy themselves, who in many cases exhibit a want of interest in the schools, but the local communities also have their sympathies chilled, in the first place, by an over predominance of the clergy in school management, and, secondly, by the excessive interference of bureaucratic action from above. The nature of this bureaucratic action has now to be described.

The kingdom of Prussia is divided into provinces, each province into departments, each department into circles or districts, each circle into parishes or communes. For the whole kingdom, the central educational authority is, of course, the minister of public worship, and medical and educational affairs. Beneath him there is a gradually descending scale of officers, for the superintendence of instruction on the system that a civil authority is always associated with clerical or scholastic affairs. Thus for the province, the president of the province is associated with a provincial school council. For the department, the prefect of the department is associated with a departmental school councillor. For the circle or district, the landrath, or district councillor, is associated with the superintendent, who is an ecclesiastic of about the same dignity as an archdeacon in England, and who supervises the inspection of schools in from twenty to forty parishes. In the parish there is the school board associated with the local clergyman, who, as we have seen, is *ex officio* school inspector and school manager.

The provincial school council, in conjunction with the president of the province, manages higher education alone.

All reports on primary instruction are sent up by the superintendents of districts to the departmental school councillor, who, in conjunction with the prefect of the department, forwards them direct to the minister of instruction. The superintendent, though an ecclesiastic, is said to act invariably in a bureaucratic, and not a clerical spirit. It may easily be supposed that, with all this network of reports radiating towards the centre, there is little scope left for local action in the matter of the common schools. Though the rate-payers furnish the funds, they have little to say on their expenditure. The schoolmasters appear to be appointed, not by the parish school boards, but in each case by the departmental school councillor. For some time there was a certain liberty left to individual masters and to local feeling in the kind of teaching to be given in the schools; but, in 1854, certain famous *Regulative*, or Minutes of the Bureau of Public Instruction, were issued, absolutely defining the subjects and manner of teaching. Of these minutes I will speak presently. They gave final extinction to anything like local and characteristic life in connection with the country schools.

In large towns they have another board called the *Schul-deputation*, or school delegacy, for the collective management of the city schools. These bodies were first created in 1808, when, under Stein's advice, every possible means was being adopted for calling forth the energies of the nation, and, amongst other things, it was thought desirable to awaken municipal life. In Berlin, the school delegacy, consisting of chosen members of the town council, have the management of all the schools, both higher and primary, within the city, except a few which are of an exceptional character. But the school delegacy has to report to the provincial council of Brandenburg, and Mr Pattison mentions that on one occasion they were reproved for too much independence, for having examined some candidates as teachers in needle-work without having sought the permission of the provincial government. In short, the central power has of late evinced much jealousy of the school delegacies, and has apparently wished to take back, or neutralise, the dangerous concession of 1808.

In Prussia the so-called "religious difficulty" has never existed. The schools of every kind are religious and denominational. The religious difficulty arises from a multiplicity of sects, and from antagonism between established and non-established churches. But in Prussia there are three leading confessions, all endowed respectively in different localities, which cover almost the entire population,—the Lutheran, the Reformed, and the Catholic. The two first are conjoined for school purposes; and thus we have the denominational proportions of population stated some little time ago, as follows:—

Protestant	.. .	64·64 per cent.
Catholic	. .	32·71 „
Other creeds	. .	2·65 „

Of these other creeds five-sixths were Jews, the remainder Dissenters—such as Baptists, Mennonites, Irvingites, &c. This phenomenon of more than ninety-seven per cent. of the population belonging to established churches may remind us of the case of Scotland, where, I believe, about eighty-eight per cent. of the population belong, if not to one establishment, at all events to one confession, without material doctrinal differences.

The Jews in Prussia, whenever congregated in sufficient numbers, have schools of their own, with their own religious teaching. If they exist in isolated families, their children attend the Christian schools, and are generally not withdrawn even from the religious teaching. They are said to look on instruction in Christianity as a piece of useful or curious information, and to be quite above the fear of conversion. In this respect they are like a certain Brahmin of Bengal, who, having attended a missionary school, reassured his caste by telling them that "he had gone through the whole Bible, and it had done him no harm."

The Dissenters are obliged to attend the public schools, but they are under the protection of a conscience clause. The authorities require evidence that the children of Dissenters are taught religion according to their own formulæ by their respective clergy. The Prussian constitution of 1851 contained the following article:— "In the ordering of public schools for the people, regard shall be had to denominational relations. The religious instruction in the people's school is under the conduct of the respective religious

bodies." The conscience clause dates back from the Prussian code of 1794, which lays down that "admittance into the public schools shall not be refused to any one on the ground of diversity of religious confession. Children whom the laws of the State allow to be brought up in any other religion than that which is being taught in the public school, cannot be compelled to attend the religious instruction given in the same." This order, however, except in the numerically insignificant case of the Dissenters, appears seldom to have been put in force. Mixed schools, where teachers of different confessions are associated together, have been tried occasionally, but have not been found successful. It has long been an established maxim in Prussia, that all schools must be denominational, and, as a rule, every child appears to find him or herself at a school belonging to his or her religious denomination.

The obstacles in the way of legislating for the instruction of the people in this country arise *in limine* from differences of opinion as to the questions of religious teaching, school management, rating, and compulsory attendance. The obstacles in the way of educational legislation in Prussia arise from differences of opinion as to the relation of Church and State to local communities. But in Prussia the difficulty is only about altering the character of a system. The system is there, and is complete enough in itself. The only question is, Could not a better and freer system be introduced? We have seen how the Prussian people, following the advice of Luther, adopted universal school attendance as a national habit; how this habit was ratified and confirmed by law in the eighteenth century; how the support of people's schools was thrown on the householders by the code of 1794; and how, by common consent, and by law, the schools have remained denominational, with a conscience clause for the benefit of a very small section of the population. Thus has Prussia, in the march of time, quietly stepped over all those preliminary and merely parliamentary difficulties, which in this country have so long prevented large numbers of the people from getting any school education at all, while Lords and Commons have been wrangling as to the exact form under which the schools were to be started.

But all this touches merely the external politics of public instruction. The question remains, What is the teaching in the

people's school when you have got it established? On this point the experience of Prussia is not uninteresting. The elementary school in Prussia was, in its origin, a catechetical instruction; it was a repetition by some subordinate ecclesiastic of the Sunday catechising of the pastor. Gradually the teaching of reading and singing was added, but only as a means to a religious end, namely, reading the Bible and singing in church. By the middle of the eighteenth century more secular elements of instruction were grafted on; and Frederick II., in 1763, orders that "the people shall be Christianly brought up in reading, praying, chanting, writing and arithmetic, catechism, and Bible history. The Prussian code of 1794 lays down that schools and universities are "institutions of the State." It prescribes the teaching of religion as a part of useful knowledge, and as tending to make good and obedient citizens. At the end of the last century the Prussian elementary schools appear to have been easy-going mechanical institutions, with nothing about them specially to call for remark. But an immense ferment in relation to them was preparing, a passionate upstirring of the whole question of popular education, endless theory and counter theory, action and reaction, the history of which constitutes a whole literature, and the effects of which have all been felt upon the character of the Prussian *Volksschulen*, which now remain like the fossilised result and record of the storms of the past.

All this commotion rose from the fervid brain and heart of one man, Henry Pestalozzi, a Swiss, who was born at Zurich in 1746. Pestalozzi was a loving enthusiast; of a most unpractical turn of mind; always embarking in visionary schemes for the good of others; of a large and noble heart, living a life of poverty and struggle himself, but always spending his whole strength in efforts for the welfare of the poor. He lived to be eighty-one years old, and long before his death he had been publicly visited and honoured by emperors, kings, and statesmen, and had seen his ideas warmly received and widely spread over the continent of Europe. Pestalozzi was much influenced in early youth by reading the "Emile" of Rousseau. In 1780 and subsequent years, after many failures in life, he began to bring out books on education. The chief of these were, "The Evening Hour of a Hermit," con-

taining educational and religious aphorisms; and "Leonard and Gertrude," a story to illustrate what might be done by a particular method of teaching children. These and other writings of his excited great attention. He had successively different schools under his management, in which he developed his system by practical experiment. Finally, at Yverdun, in the year 1805, he had obtained care of an institution which has now become a classical name in the history of pedagogy.

Pestalozzi's fundamental idea was that the children of the poor, in a public school, should be taught as if by an affectionate mother, who entered into all their feelings, and anticipated their difficulties. His conception was that primary instruction should not consist in giving knowledge verbally, mechanically, or by rote, but in drawing out the powers of the child. He laid it down that no child should be taught anything which it could not understand. The first development of this idea resulted in lessons upon form, number, and language. At Yverdun, Pestalozzi would carry his class through a lesson of the following kind:—Pointing to the wall, he would say,—

"Boys, what do you see?"

(*Answer*) "A hole in the wainscot."

"Very good; now repeat after me—

"I see a hole in the wainscot.

"I see a long hole in the wainscot.

"Through the hole I see the wall.

"Through the long narrow hole I see the wall.

"I see figures on the paperhangings.

"I see black figures on the paperhangings.

"I see round black figures on the paperhangings.

"I see a square yellow figure on the paperhangings.

"Beside the square yellow figure, I see a black round figure.

"The square figure is joined to the round one by a thick black stroke." And so on.

It was said that Pestalozzi used to shout out sentences of this kind without any explanation, and was echoed in chorus by the class. It is true that words in this way became associated with impressions of the sense. But if this were all, we should say that Pestalozzi was incapable of developing his own theoretical idea.

A trace of such teaching reached this country in the shape of the so-called "object lessons," which, without much fruit, were once in vogue in England.

But the Pestalozzian method had in reality far greater results. A swarm of enthusiastic assistants, perhaps more clear-headed than their master, came to serve under him; and by them there was worked out—

(1.) All sorts of methods for conveying in an easy manner to the child the arts of spelling, reading, ciphering, and so on.

(2.) The practice of a sort of Socratic dialogue, for developing the intelligence of the class upon the subject of the lesson, whatever it might be.

(3.) The idea of pedagogy as a science, based upon psychological *data*.

(4.) The idea that religion, which with Pestalozzi was made the basis of all, must not be taught dogmatically and confessionally, but rather universally; in short, that the first teachings must be of natural religion, and not of the religion of any Church.

All this was new, and it had a peculiar fascination for several of the greatest minds of the age. When, in 1806, Prussia was crushed by Napoleon, and went through afflictions strikingly analogous to those that have now befallen France, Stein and Fichte, the statesman and the philosopher, both earnestly proclaimed that the moral energies of the nation must be regenerated by the universal adoption of the Pestalozzian ideas. Pestalozzian schools were established over the country, and in subsequent years the system was thoroughly exploited; all its strength and weakness were brought to the full light of trial and experience.

The result of fifty years' exhibition and discussion of the Pestalozzian system has been as follows:—

(1.) There is a considerable residuum in the shape of excellent technical methods for teaching the elements of knowledge. Thus each child is taught to read easily, alone, within twelve months. The old plan of first learning the names of the letters, and then spelling, is abandoned. In arithmetic, the child is taken through the operations of the four rules, both in integers and fractions in the tens, before he reaches the hundreds. The magnitudes to be dealt with form the only distinction between the classes in arith-

metic. These and other methods are the result of the immense attention which has been bestowed on the question of primary teaching.

(2.) Public opinion has pronounced against much that was characteristic of the Pestalozzian system. From the principle that children should be taught nothing that they could not understand, there was deduced the practice of much abstract and formal lecturing, totally unsuited to children from six to nine years of age. Thus, lessons on the theory of number were made to precede empirical teaching of arithmetic. While much stilted talk was used both about the children and to the children, it was found that, in many cases, they were suffered to go through school without learning to read and write. A general reaction set in against the idea of intellectual training in common schools.

(3.) This tendency of public opinion was taken up and ratified by the Government. In October 1854, *Regulative*, or Minutes from the Office of Public Instruction in Berlin, were issued, which bear a close analogy in some points to the revised code of Mr Lowe. The object of these minutes was to restrict the teaching in elementary schools to a few humble and necessary subjects, and to ensure these subjects being efficiently taught. In direct opposition to Pestalozzi, the *Regulative* proceeded on the principle that, in an elementary school, it is *not* the object to develope the child's reasoning faculties, or to give him knowledge, but only to give him the power of doing certain things;—*Können*, and not *wissen*, was to be the result to be produced. The schools were to turn out the children in possession of the actual capacities (*fertigkeiten*) of reading, writing, and ordinary ciphering, and everything outside of this range was to be sternly excluded. Thus the children were on no account to learn grammar, as this is an abstract, logical thing, suited to the high school; whereas, in an elementary school, children should learn to use their own language correctly by practice, and not by rules. Even mental arithmetic was to be excluded, as being a needless fatigue of the brain. Of secular subjects, in addition to the three R-s, only singing was as a general rule to be taught, for the sake of practising the voice and ear. Only church tunes and national songs were to be permitted, the words being previously well studied and explained. History and geography were

discouraged; if taught at all, they must be limited to *Heimathskunde*, or information about the child's native land. Drawing, if introduced, must be confined to linear freehand copying from the flat.

Religion remained an essential and prominent element for the people's schools, but the *Regulative* made a great change in regard to the mode of imparting it. Under the Pestalozzian system, religion had been taught not confessionally, but universally; not as a matter of Church formulæ, but in a free and spiritual way, which, of course, depended for its characteristics very much on the individual master. When the time for confirmation arrived, the clergyman would find the children furnished with ideas, more or less orthodox, of natural religion and of Christianity, but perhaps never having seen the Church Catechism, and the labour would devolve on him of making them learn this. It appeared to the Government that the schools, though denominational in their foundation, were too independent of the Church in their religious teaching. The *Regulative*, by one stroke, altered all this. They laid down exactly what was to be taught in the shape of religion, namely, some fifty hymns were to be learnt by heart, the whole of the gospel portions which are read in the Lutheran churches were to be committed to memory, and the Catechism (either Luther's or the Heidelberg) was to be learned off by rote, without any explanation. All explanation of the doctrine contained in it was to be reserved for the pastor, when the time of confirmation drew nigh. By these rules, the relative positions of the clergyman and the schoolmaster were completely subverted. All the charm of teaching religion to the children was taken away from the master, whose task was, in this respect, made mechanical, while he himself was made completely subordinate to the clergyman.

The minutes on religious teaching had, doubtless, a political and ecclesiastical motive, and a reaction against them is possibly in preparation. Those regulating the secular subjects in the people's schools are a specimen of the Prussian Government, as a powerful decisive will, proposing to itself certain definite ends, and going straight at these ends without compromise or collateral considerations.

In the case of the elementary schools, there can be no doubt that the end aimed at is attained; for the schools embrace the entire population, and the result is, that the children of every

peasant and labourer have, as a matter of course, the arts of reading, writing, and cyphering, know the Church formulæ and a good deal of the Bible, and can take part in singing a hymn or national chorus.

But I think that one misses in these schools anything calculated to raise the intelligence of the people, anything analogous to the influence of the parochial schools of Scotland. The repression of the high-flown Pestalozzian aspirations has been too absolute. The definition of an elementary school has been too logical. There is nothing to lead on towards the higher grades of education. The people's school seems sharply separated off, and to give the children of the people no encouragement or opportunity to rise. One proof of this may be found in the fact that pupils who, at fourteen years of age, have passed eight years in the primary school, and who then have two years further preparation under a public schoolmaster or clergyman, are, at sixteen years of age, commonly unfit to enter upon the very simple curriculum of the training college.

It may be asked whether industrial or technical instruction does not form part of the Prussian system? But in the ordinary people's school nothing of this kind is attempted. The Prussian Educational Department conceives that it has a particular function to discharge for the people, and of this it acquits itself, and does no more. It is argued that seven or eight years' schooling, at the rate of twenty-six hours per week, is not more than sufficient for imparting to all with certainty the elements of common knowledge and religion, and that any attempt at technical instruction would only interfere with this; and everything technical must be learnt practically, or otherwise, after the age of fourteen. One means of supplementing the meagre results of the people's schools, consists in the *Fort-bildungsanstalten*, or "improvement schools." These exist generally in the shape of evening classes in mathematics, French, &c., for youths and adults. They have not been organised systematically, and even if they were, could hardly supply the want of a more early awakening of the intellect.

But, of course, many children, and some even of the poor, quit the elementary school at nine years of age, to enter on the course of higher instruction.

In all the departments of higher instruction, Prussia seems to me to be distinctly ahead of England, and still more so of Scotland. But I have already take up so much of your time, that I must now confine myself to a few aphorisms on this subject. In Prussia education is considered to be so completely a matter of national concern, as always to call for the supervision of the State. No man may start a private school, whether primary, middle, or higher, without a license from the educational office. And this license is only given after the passing of prescribed examinations. The too common charlatanry of private schoolmasters in England is thus avoided. A useful censorship of schoolbooks is exercised by the minister of instruction. By this the crotchets of schoolmasters in the use of eccentric and useless books are checked.

The minister of instruction is not only a man of science or learning himself, but he has the advice of councillors of the highest scientific and literary reputation. The opinions of such a central board on questions of higher instruction are not merely bureaucratic edicts, but constitute a valuable intellectual guidance.

With regard to resources, the following distinction is to be observed in Prussia. The elementary schools get very little money from Government, only a small contribution from school fees, and the great bulk of their expenses from parish and municipal rating. The support of the higher schools of all kinds appear to be as follows :—

From Fees, a proportion of . .	5·4
From Municipal assignments, . .	2
From Grants by Government, . .	1·6
From Endowments, . . .	1·

Thus the fees of scholars pay considerably more than half the cost of the higher schools. Municipal contributions amount to one-fifth, and grants from general taxation to nearly one-fifth, endowments to one-tenth. Fees in the high schools are often remitted wholly or partially on the ground of the circumstances of the parents. Out of about 90,000 scholars attending the superior schools of Prussia, about 20,000 appear to be wholly or partially free scholars.

The higher education goes in Prussia, the more entirely does it

become recognised as a proper object for State maintenance. Thus the universities, so far as their own resources fall short, are fully supplied by the Government. The University of Berlin, in the year 1864, had an income of about L.30,000. Of this, L.24 only was the interest of funded property of the University; L.1133 was the amount of entrance and examination fees; L.28,842 was the grant from Government.

If we compare with this the University of Edinburgh, we find the income for the current year to be L.20,351, of which L.4153 are fees of various kinds, L.9869 funds from private endowments and other sources in the hands of the Senatus, L.6329 parliamentary grants. This shows how comparatively small is the proportion of State assistance to our University.

The higher schools of Prussia consist of two distinct branches—the *Gymnasien*, or grammar schools, with their *Pro-Gymnasien*, or preparatory grammar schools, and the *Real-schulen*, or scientific schools, with the "higher burgher schools" in preparation for them. The *Gymnasien* are, of course, the product of the Middle Ages, the Renaissance, and the Reformation. The *Real-schulen* sprang from the modern protest on behalf of science against the predominant claims of classics. The Gymnasium is a first-rate classical day school, with a time-table of 30 hours per week. It has six classes, *Prima* being the highest. The 30 hours in *Prima* are thus allotted:—Religion, 2; German, 3; Latin, 8; Greek, 6; French, 2; History and Geography, 3; Mathematics, 4; Physics, 2. Besides these school hours there is extra-time instruction in singing and gymnastics; and those who propose subsequently to study theology or philology in the University are required to learn Hebrew, also in extra hours.

The time-table, though thus definitely prescribed, is not rigidly adhered to; for promising pupils in the first class are allowed a good deal of liberty for private study in lieu of the stated lessons.

Many enter the Gymnasium irrespective of an intention to proceed to the University, for the sake of the privileges which it holds out. For, those who have gone through the classes and passed the leaving examination, besides qualifying for the public service, are allowed to serve for one year as volunteers in the army, instead of three years according to the ordinary course.

But yet it is endeavoured to keep up a thoroughly intellectual atmosphere in the *Gymnasiens*. The Prussian Government lays it down that culture for its own sake, and not with any premature regard to the practical exigencies of life, is to be the object of these schools. And it expressly forbids that those who propose to enter the army as a profession, should abate any of the higher classical studies of the first class. This is certainly very different from the principle adopted in English public schools.

The crowning result, and the most distinctive feature of the *Gymnasium* is the *abiturienten-examen*, or leaving examination. The certificate of having passed this examination is, of course, ardently desired by the pupils, as it is the key to entry into any of the learned professions, and gives important exemption in military service. This being the case, it may be affirmed that in this country an analogous examination would often lead to over-strenuous preparation on the part of the pupils when the time of the examination drew nigh. But the Prussian Government takes the greatest care to obviate a result which they would deem utterly unsatisfactory. They lay down the strictest rules, both in general terms and in detail, to prevent the examination being of a kind for which any special preparation, spasmodic efforts, or cram would be of any avail. It is by no means to turn upon the learning up of names, dates, and isolated facts; but it is to exhibit (as the educational minute says) "the slowly ripened fruit of a regular and contant industry throughout the whole school course."

With this object, one of the grounds for the certificate is made to consist in a record of the pupil's work throughout perhaps the nine previous years in all the classes of the Gymnasium from *sexta* to *prima*. In addition to this, the examination is to show how much of the school study has really been assimilated by the pupil, and has become part of himself. The Prussians are much wiser than some other countries in the matter of examinations. They always keep in view the exact end they are aiming at. In the *abiturienten-examen* they don't want a paper, but a man; and they certainly adopt the best means of testing the man's real acquirements and deserts, when, on the one hand, the examiners have before them a continuous record of his previous work for years, and, on the other hand, submit him to such general exercises in

languages and mathematics as show in each subject what amount of proficiency he has really available. The examiners consist of the upper masters of the school itself, with certain commissioners from the Government associated with then. Persons who have been brought up in private high schools, and who wish to proceed to the University, must present themselves at the examination of the Gymnasium, where they will be equitably examined. But on the whole the public schools are most popular in Prussia, and the scholars of private schools are quite in a minority. The paper work of the examination occupies a week. The chief subjects are —(1.) An essay in German, which is intended to exhibit general culture, taste, and correct writing. It is analogous to the English composition in the Indian Civil Service competition. (2.) A Latin essay. (3.) A piece of simple Greek prose to be written. (4.) A translation of German into French. (5.) Two geometrical and two arithmetical problems to be solved. A *viva voce* examination follows, consisting of translation from pieces, not prepared in class, of the Latin and Greek authors, questions in metre, mythology, history combined with geography, and antiquities; conversation in Latin; examination in Bible history and the Church Catechism; and for future philologists and theologians, an examination in Hebrew.

The certificate which each candidate receives is marked either "insufficient," "sufficient," "good," or "excellent." The mark "insufficient" is meant to indicate unripeness for the University. The pupil receiving it is recommended to prolong his attendance at school, or to seek some other career in life for which University study is not required. But if he and his parents wish it, he may still enter the University, with his certificate of "unripeness." In that case he will be restricted to the faculty of philosophy, and not allowed to enter any learned profession, unless he can, by subsequently presenting himself at the gymnasial examination, obtain a certificate of being "ripe;" and in the meantime he will be debarred from holding any University scholarships or stipends. The holders of favourable certificates, with "good" or "excellent" for their examination, and a full record of previous conduct and performances, carry with them an important testimonial for the outset of life.

In all these arrangements of the leaving examination of high schools, we see, I think, that Prussia dares to be thorough in a matter of this kind. She insists that high schools should do their work, and by giving the universities, the public service, and the learned professions an organic connection with these schools, she makes it a very serious matter for all the pupils to take advantage of their opportunities. Without any apparent strain upon the pupils, she succeeds in obtaining a higher standard of results from school boys than is implied in the ordinary M.A. degree of the Scotch universities, or the ordinary B.A. degree of Oxford or Cambridge.

Of the *Real-schulen*, or scientific schools, I have not much to say. Started originally more than a hundred years ago, it is only within the last fifty years that they have had a considerable development. Of the 90,000 pupils attendant on secondary schools in Prussia, about 30,000 appear to go to the *Real-schulen* or their preparatories. These schools do not prepare for the universities, but for business, certain departments of the public service (such as architecture or mining), and for the Polytechnic College.

The time-table for *Prima* in a *Real-schule* consists of thirty-two hours, made up as follows:—Religion, 2; German, 3; Latin, 3; French, 4; English, 3; Geography and History, 3; Natural Sciences, 6; Mathematics, 5; Drawing, 3. Latin, however, is not insisted on, and a liberty is left to the school delegacy of adjusting the subjects in some degree to the necessities of the immediate neighbourhood, with reference either to particular languages or particular industries, that may exist. A suitable leaving examination is prescribed, qualifying the holders of certificates for military exemption and for the public service.

An eminent authority, Dr Jäger, told Dr Matthew Arnold that the *Real-schulen* were not considered successful institutions. He said that the boys in corresponding classes of the classical schools beat the *Real-schule* boys in subjects which both do alike, such as history, geography, German, and even French, on which the *Real-schule* boys spend much more time. Dr Jäger assigned as the cause for this result that classical training strengthens a boy's mind more than modern or scientific teaching. I confess, however, that I think the comparison, as stated, not quite complete,

as in matters not connected with language and history the *Realschule* boys might be found to have faculties of observation and deduction to which the classical boys would be strangers. I merely state what has been said.

Turning now to the universities of Prussia, we find ourselves in the region of pure unfettered science. The *abiturienten-examen* of the classical schools gives the universities such a starting ground in the thorough previous education of all the students who matriculate, that they are able to commence the treatment of all subjects on a high scientific level, in confidence that such a mode of treatment will be followed and understood.

The appointments of professors are invariably made, so far as I can learn, on the grounds of greatest scientific eminence. The appointments are all in the hands of the Crown—that is, of the minister of instruction. When a vacancy occurs, the faculty to which the chair belongs sends up a short leet of names to be recommended to the minister, and from these he generally makes the appointment. But I believe that the name chosen is always that of the man whom previous public performances and general opinion in the scientific world have designated for the place. I believe that anything like political or theological bias in the appointment of professors is unheard of. Other personal considerations (which might be more plausibly entertained) are also omitted, such as power of clear exposition and capacity for managing a class. Hence it may happen that the professor, when appointed, is obscure in style and unattractive as a lecturer; but the students have, at all events, the feeling that in him they have the greatest authority that could be found on the particular subject. And there is in German universities a general consciousness that it is better to have the last and most reliable results in science than to have a popular exposition of what is old and perhaps exploded. The professor has a fixed salary from Government, frequently amounting to L.350 or L.400 a year, in addition to a share of examination fees and the fees of his class. But he is bound to lecture free of charge twice a week. The fees in theology or philosophy are about 17s. for the six months. In the medical classes they go as high as L.1, 14s. 5d. for the course. Several professors have altogether an income of from L.1000 to L.1500 a year, which, in proportion to

ordinary rates of expenditure in Germany, is something considerable. We all know that the headmasters of Eton and Rugby realise L.4000 or L.5000 per annum, which is probably superior to most university emoluments within the United Kingdom. But nothing of the kind occurs in Prussia; the highest schoolmasterships are below, both in rank and emolument, the ordinary run of professorships. The best school appointment in Prussia appears to be the rectorship of the *Schul-Pforta*, an endowed gymnasium in Prussian Saxony; to this L.300 per annum and a house are attached. The professors, being fairly endowed by Government, are far from being sheltered from competition by any kind of monopoly. The State can always appoint any eminent man as full professor, even in a faculty which has already its full complement. Then, secondly, the State at its pleasure appoints extraordinary or assistant professors, who have a small salary, their chief reliance being on fees. Thirdly, the Faculties appoint as *Privat-docenten* persons who can prove their fitness. The *Privat-docenten* appear not to fulfil the functions of what we should call tutors, but rather to be analogous to our extra-academical lecturers in the Medical Faculty. The *Privat-docenten* and the extraordinary professors form a reserve of men, establishing their reputations, from whom the future full professors will be chosen. Before the beginning of the session a harmonious arrangement is made between the professors, extraordinary professors, and *Privat-docenten*, in a Faculty, as to the subjects on which each is to lecture, so as to cover the whole field of instruction proper to the Faculty. The dean then publishes the programme, and the only restriction is that the fees must be uniform.

There is, in short, absolute liberty of teaching to those who can prove their competent knowledge of any subject; and there is equal liberty of learning, for no student is obliged to attend any particular courses, or number of lectures, with a view to his degree. All that general culture which we endeavour to ensure by our Arts curriculum is provided in Prussia beforehand by the *abiturienten-examen*, and the student is considered fit to choose absolutely for himself his own University curriculum. In the professional Faculties he, of course, cannot dispense with instruction in all the separate branches; but in the Faculty of Philosophy, which answers to our

Faculty of Arts, and embraces the humanities and the mathematical and natural sciences, the student is allowed to choose any two subjects he likes for his final examination; and if he passes in these, he gets his degree as Doctor of Philosophy. To pass, however, in any subject is supposed to imply, not a schoolboy preparation, but a manly mastery of the whole subject. For instance, in order to pass in Greek and Latin philology a student would be called on to revise the readings in some Greek or Latin book, with scholarly reasons for all his opinions on each point, and, in addition, to show, *viva voce*, a complete knowledge of classical literature, philology, and antiquities. The liberty allowed to students is doubtless often abused. In a recent life of the Count von Bismarck it is mentioned that, while attending the University of Berlin, he fought innumerable duels, and only attended one lecture. That lecture was by the eminent Professor Savigny; but Bismarck, thinking that he did not gain within the hour as much information as would suit his purposes, abandoned the course, and applied himself to a *repetitor* or crammer, by whose assistance he succeeded in passing the examination of the Law Faculty.

On the whole, there is probably not so much industry among the students of a German as of a Scotch University; but there is far more than at Oxford or Cambridge. And whenever industry exists, being based on more complete previous preparation, and being in relation to really scientific lectures, it is probably of a higher and more fruitful kind than can be found among the students of Great Britain.

Still, complaints are made against the Prussian university system. One of these is, that the students are too exclusively engaged in taking notes of lectures, and that they have too little practice of their creative faculties. The prejudicial effects of this may, perhaps, be traced in the want of the graces of style which characterises to so great an extent most German books.

Another complaint is, that the students. though systematically prepared up to entrance into the university, are afterwards left without sufficient guidance as to the order in which they should take up successive subjects.

It is quite possible that Prussia, which honestly and thoroughly desires the best in education, may descend a little from the clouds

in its university system, and deign to adopt something like the Little-go or Moderations examination of the English universities, though such an examination in Prussia would be, of course, on a distinctly higher level. Prussia might, perhaps, with advantage curtail a little the liberty of her universities, and increase a little the liberty of her primary schools, in respect both of studies and management. She might allow a more easy and natural connection than appears to exist between the primary school and higher education. She would like also to see a gradual relaxing of the leading strings of Government, and a greater development of cultivated local energies. It would be a great misfortune for the new-born German empire if military successes should be found to have intensified the centralising forces in all the affairs of national life. The Liberals appear sanguine that this will not be the case. But a struggle on questions of internal policy may very likely succeed the conflicts of the war. In the meanwhile, on the educational question Germany and England hold positions the very opposite of each other. In Germany there is the idea of what is wanted, and a universal carrying out of that idea. But too much comes from the central power. There is a deficiency of communal life and independent individual action. The question with Germany is how to shift, without losing, the motive power. In England there is abundant local action and vitality, but a deficiency in cultivated guidance for that action. There is with us an immense lee-way to make up, both in overtaking, with primary instruction the masses of the people, and also quite as much in regulating and defining the aims and the method of secondary and university education. The great question for England in this matter seems to be, first, how to get over religious difficulties in the way of primary instruction; and, secondly, how to obtain a sufficiently enlightened guidance for our higher education, without adopting, which all ought to deprecate, anything like a bureaucratic system.

On the Physiology of Wings: being an Analysis of the Movements by which Flight is produced in the Insect, Bat, and Bird. By James Bell Pettigrew, M.D., F.R.S. Communicated by Professor Turner.

(*Abstract.*)

(Received 2d August 1870.)

In the present memoir the author enters very fully into *the figure-of-8 wave movements*, described by the wing in space, to which he first directed attention in March 1867.* He has adduced the experiments with *natural* and *artificial wings*, on which his description was originally based, and has shown, by the aid of original models and a large number of diagrams and drawings, that *artificial wings* can be made to approach indefinitely near to *natural ones*, not only in their structure, but also in their movements. He further points out that the fins and tail of the fish—the flippers and caudal extremity of the whale, dugong, manatee, and porpoise, and the flippers of the seal, sea bear, walrus, and turtle—bear a close analogy to wings, and ought to be studied in connection with them. As further proof that the wing describes a figure-of-8 wave-track in flight, the author cites the results announced in February 1869 by Professor J. E. Marey, of Paris.†

* *Vide* "The Various Modes of Flight in Relation to Aëronautics;" by the Author in the "Proceedings of the Royal Institution of Great Britain for March 22, 1867;" also his memoir "On the Mechanical Appliances by which Flight is attained in the Animal Kingdom," read to the Linnean Society of London on the 6th and 20th of June 1867, and published *in extenso* in the 26th volume of their Transactions, a large number of woodcuts and engravings being specially devoted to the elucidation of the figure-of-8 wave track made by the wing as observed in the flight of the insect, bat, and bird.

† "Revue des Cours Scientifiques de la France et de l'Etranger." Professor Marey, in a letter addressed to the French Academy, under date May 16, 1870, fully acknowledges the author's claim to priority (as regards himself) in the discovery of *the figure-of-8 wave movements made by the wing in flying*. M. Marey, in the letter referred to, states ("Comptes Rendus," page 1093, May 16, 1870), "J'ai constaté qu' effectivement M. Pettigrew a vu avant moi, et représenté dans son Mémoire, la forme en 8 du parcours de l'aile de l'insecte: que la méthode optique à laquelle j'avais recours est à peu près identique à la sienne . . . . . je m' empresse de satisfaire à cette demande légitime, et je laisse entièrement la priorité sur moi, à M. Pettigrew relativement à la question ainsi restreinte."

Professor Marey, by employing a sphygmograph similar to that used for ascertaining the state of the pulse, succeeded in causing the wings of insects and birds to register their own movements. He says:—"But if the frequency of the movements of "the wing vary, the *form* does not vary. It is invariably the same; "it is always *a double loop, a figure of* 8. Whether this figure be "more or less apparent, whether its branches be more or less equal, "matters little; it exists, and an attentive examination will not fail "to reveal it." *

The subjoined are a few of the results obtained by the author in the course of his numerous observations and experiments:—

The wing is of a generally triangular form. It is finely graduated, and tapers from the root towards the tip, and from the anterior margin towards the posterior margin. It is likewise slightly twisted upon itself, and this remark holds true also of the primary or rowing feathers of the wing of the bird. The wing is convex above and concave below, this shape, and the fact that in flight the wing is carried obliquely forward like a kite, enabling it to penetrate the air with its dorsal surface during the up stroke, and to seize it with its ventral one alike during the down and up strokes. The same remark applies to the *remiges* or rowing feathers of the wing of the bird.

The wing is moveable in all its parts; it is also elastic. Its power of changing form enables it to be wielded intelligently, even to its extremity; its elasticity prevents shock, and contributes to its continued play. The wing of the insect is usually in one piece,† that of the bat and bird always in several. The curtain of the wing is continuous in the bat, because of a delicate elastic membrane which extends between the fingers of the hand and along the arm; that of the bird is non-continuous, owing to the presence of feathers, which open and close like so many valves during the up and down strokes.

The posterior margin of the wing of the insect, bat, and bird, is rotated *downwards and forwards* during extension, and *upwards*

* Revue des Cours Scientifiques de la France et de l'Etranger, p. 252. 20th March 1869.

† The wings of the beetles are jointed, so that they can be folded up beneath the elytra or wing cases.

and backwards during flexion. The wing during its vibration descends farther below the body than it rises above it. This is necessary for elevating purposes.

The distal portion of the posterior margin of the wing of the insect is twisted in a downward and *forward direction* at the end of the down stroke, whereas, at the end of the up stroke it is twisted downwards and *backwards*. The proximal portion of the posterior margin always assumes a reverse position to that occupied by the distal portion, so that the posterior and anterior margins of the wing are not in the same plane, and in certain situations the two margins appear to cross each other. What is here said of the insect's wing applies equally to the wings of the bat and bird.

The wing during its vibrations *twists* and *untwists*, so that it acts as a reversing reciprocating screw. The wing is consequently a screw *structurally* and *functionally*.

The blur or impression produced on the eye by the rapidly oscillating wing *is twisted upon itself*, and resembles the blade of an ordinary screw propeller.

The twisted configuration of the wing and its screwing action are due to the presence of *figure-of-*8 *looped curves* on its anterior and posterior margins; these curves, when the wing is vibrating, reversing and reciprocating in such a manner as to make the wing change form in all its parts. The curves in question are produced to a great extent by vital movements, independently alike of the elasticity of the wing and the reaction of the air. They can, however, be produced by the latter agencies likewise. The change and reversal of the curves occurring on the anterior and posterior margins cause the different portions of the wing to strike at various angles during the down and up strokes.

The angles which the different parts of the wing make with the horizon are greatest towards the root, and least towards the tip of the wing. The angles are, in fact, adjusted to the speed at which the different portions of the wing travel—a large angle with a low speed giving the same amount of buoying and propelling power as a small angle with a high speed.

The speed attained by the tip of the wing is always very much higher than that attained by those portions nearer the root—the

root corresponding to the *short* axis of rotation. (The *long* axis of rotation runs along the anterior margin of the wing.)

The angles which the wing makes with the horizon are increased during the down stroke, and decreased during the up stroke, the posterior margin of the wing being screwed down upon the air during the down stroke to increase the elevating and propelling power of the wing, and unscrewed or withdrawn from the air during the up stroke to afford support, and assist in propulsion.

The wing, in virtue of the variations of inclination of different parts of its surface, acts as a true kite during both the down and up strokes, *i.e.*, it flies down and up alternately in such a manner as to keep its ventral concave or biting surface always closely applied to the air. The wing is, therefore, effective during both the *down* and *up strokes*, so that it is a mistake to regard the down stroke as alone contributing to flight. In reality the down and up strokes are parts of one movement, the wing describing first a looped and then a wave track.

The tip of the wing in especial acts as a kite during the up stroke, the kite being inclined upwards, forwards, and outwards.

The kite formed by the wing differs from the boy's kite in being capable of change of form in all its parts. The change of form of the wing is rendered necessary by the fact, that the wing is articulated or hinged at its root (short axis), its different parts, as a consequence, travelling at various degrees of speed in proportion as they are removed from the axis of rotation. It is also practically hinged along its anterior margin (long axis), so that the tip travels at a higher speed than the root, and the posterior margin than the anterior. The compound rotation and varying degree of speed attained by the different parts of the wing has the effect of twisting the wing upon its long axis, and producing a variety of kite-like surfaces calculated to operate effectually upon the air, whatever the position of the wing may be.

The wing, when the flying animal is fixed or hovering steadily before an object, describes a figure-of-8 wave track in space,—the figure-of-8, when the animal flies in a horizontal direction, being opened out or unravelled to form first a looped and then a waved track.

In horizontal flight the wing describes a series of large waves or

curves, the body describing a series of smaller and opposite curves, the wing always rising when the body falls, and *vice versa.* The descent of the wing in this manner necessitates the elevation of the body, and the descent of the body contributes to the elevation of the wing.

The wing elevates the body when it descends, and the body, when elevated, falls forwards in a curve, and so contributes to the elevation of the wing. This arrangement draws the wing forward upon the air during the up stroke, and opposes the direct downward action of gravity by presenting the concave or biting surface obliquely to the air in the direction of the travel of the body. The under surface of the wing is thus made to act as a true kite during the up stroke.

The wing is urged at different velocities, the power applied being much greater during the down stroke than during the up one. The power is also greater at the beginning of the down and up strokes than towards the termination of those acts. The variation in the intensity of the driving power is necessary to slow the wing towards the termination of the down stroke, to prepare it for the up stroke, and to afford the air an opportunity of reacting on the under surface of the wing, to the elevation of which it contributes. The wing is elevated more slowly than it is depressed, and allows the body time to fall downwards, the fall of the body assisting in elevating the wing relatively to the bird. The wing, the air, and the weight of the body, are consequently active and passive by turns.

The wing is depressed by voluntary muscular efforts. It is elevated by vital, and mechanical acts, viz., by the contraction of the elevator muscles and elastic ligaments, by the reaction of the air called into play by the fall and forward travel of the body.

If the wing is in one piece, it is made to vibrate figure-of-8 fashion in a more or less *horizontal direction.* It thus attacks the air by a series of zig-zag movements, very similar to those performed by an overloaded dray-horse when ascending a hill. If the wing is in more than one piece, it is made to oscillate in a more or less *vertical direction ;* the wing, under these circumstances, being usually closed during the up stroke and opened out during the down stroke. The wing is closed and its area diminished during the up stroke, expressly to avoid the resistance of the air.

The wing of the insect is, in some cases (the wasp, for instance), folded upon itself during the back stroke to avoid the resistance of the air; in other cases, when two pairs of wings are present (the butterfly, for example), the first pair of wings is made to overlap the second pair for a similar purpose.

When the wing is in one piece, and made to vibrate in a more or less horizontal direction, it is followed in its passage from right to left by a current which the wing meets in its passage from left to right. When the wing passes from left to right it is followed by a current which the wing meets in its passage from right to left, and so on. The wing has therefore the power of creating the current on which it rises.

When the wing is in several pieces, and made to vibrate more or less vertically, one portion of the pinion (during the acts of extension and flexion) makes a current which another portion utilises. Thus the tip and root of the wing (hand and arm) make a current during extension on which the middle part of the wing (fore-arm) acts during flexion, and the reverse. This arrangement begets a cross pulsation, and extends in the bird even to the primary and secondary feathers. The wing may thus be said to rise upon a whirlwind of its own forming.

The wing has the power of producing artificial currents, and of utilising and avoiding natural currents, so that it is equally adapted for flying in a calm and in a storm. As the wing (or parts of the wing) strikes in opposite directions, it in this manner reciprocates, the down stroke running into and contributing indirectly to the efficacy of the up stroke, and the reverse. The down and up strokes consequently form one continuous act, and neither is complete without the other. The down stroke produces the current on which the wing operates during the up stroke, and *vice versa.*

The reciprocation of the wing is most perfect when the animal is fixed in one spot, and least perfect when it is flying at a high horizontal speed. It is, however, a matter of indifference whether the wing attacks the air or the air attacks the wing, so long as a sufficient quantity of air is worked up under the wing in any given time.

The wing of the bat and bird are drawn towards the body and flexed at the termination of the down stroke to destroy the

momentum acquired by the pinion during its descent, and to prepare it for making the up stroke. It is elevated as *a short lever* to avoid the resistance of the air, and pushed away from the body or extended towards the end of the up stroke to prepare it for making the down stroke. It is depressed with great energy as *a long lever*, and hence the greater elevating and propelling power of the down as compared with the up stroke.

When the bat and bird are stationary, the tip of the wing, from its alternately darting out and in, and forwards and backwards, during extension and flexion, and during the down and up strokes, describes an ellipse, the axis of which is inclined obliquely *upwards* and forwards. When the bat and bird are progressing at a high speed, the axis of the ellipse is inclined obliquely *downwards* and forwards, the ellipse itself being converted into a spiral and then a wave line. The outward and forward (extension) and inward and backward (flexion) play of the pinion contributes to the balancing power of the bat and bird, as it augments the horizontal area of support.

The wing of the insect is recovered or drawn towards the body, and that of the bat and bird recovered, flexed, and slightly elevated by the action of elastic ligaments. Those ligaments, by their contraction, conserve and interrupt muscular efforts without destroying continuity of motion.

The elastic ligaments are in many cases furnished with muscular fibres, and are most highly differentiated in those animals whose wings vibrate the quickest.

The primary, secondary, and tertiary feathers of the wing of the bird are geared to each other by fibrous structures in such a manner that the feathers are made to rotate in one direction during flexion, and in another and opposite direction during extension. The double rotation of the feathers in question confers a distinctly valvular action on the wing of the bird.

The under surface of the wing of the bat and bird is thrown into a beautiful arch during extension and the down stroke, the arch being so formed that its tension increases according to the pressure applied.

The wing is inserted into the upper part of the thorax, and balances the body by playing alternately above, beneath, and on a

level with it. When above the body, the latter is suspended from the wings as from a parachute. When beneath the body, it is suspended from the top of a cone formed by the wings, and when on a level with the body, the latter is placed in the centre of a circle described by the rapidly oscillating wings. The body is suspended from the wings very much as a compass set upon gimbals is suspended.

The wing balances the body in consequence of its travelling at such a speed as enables it to convert the area mapped out by its vibrations into what is practically a solid basis of support.

The wing, whether in one piece or in many, rotates upon two centres, the one centre corresponding to the root of the wing (short axis), the other to the anterior margin (long axis). The rowing feathers have a similar compound motion. This mode of action of the wing is intimately associated with the power it enjoys of alternately seizing and evading the air, of producing artificial currents, and of utilising artificial and natural currents.

The wing is cranked slightly forwards, a small degree of rotation of the anterior margin being followed by a very considerable sweep of the posterior margin.

The wing area is greatly in excess of what is absolutely necessary, and as much as four-sixths may be removed in certain insects (the common blow-fly, *e.g.*), without destroying the power of flight. The wing area may also be considerably reduced in birds without in any way impairing flight. This shows that elaborate calculations of wing area, in relation to weight of trunk, must prove futile, unless the rapidity with which the wing vibrates and the state of the air are also taken into account.

Weight is necessary to the flight of the insect, bat, and bird, as at present constructed. If flying creatures were lighter than the air, the wing would require to be twisted completely round as in the auks and penguins, so that the under ventral or concave surface would strike from below upwards, instead of from above downwards.

In aërial flight the under or concave surface of the wing is applied *from above*, whereas in subaquatic flight it is applied *from below*. The scull, like the subaquatic wing, is applied from below, so that the analogy between the aërial wing and the oar as employed in sculling is more apparent than real.

A diving bird which flies under the water *is lighter than the water*, and flies *downwards*. A bird which flies in the air *is heavier than the air*, and flies *upwards*. Relative levity and weight are therefore necessary to the diving and flying bird as at present constituted.

Weight, when associated with or operating upon wings, contributes to horizontal flight. A flying animal, when it drops from a height with expanded motionless wings, does not fall vertically downwards, but downwards *and forwards*, the wings converting what would otherwise be a vertical fall of the body partly into *forward travel*. The weight of the body thus to a certain extent relieves the muscular system from excessive exertion. If a sufficient breeze be blowing, the weight of the trunk and the breeze upon the wings operating conjointly are sufficient to keep the body of the animal in the air for protracted periods. This is well seen in the case of the albatross, which can sail about for an hour at a time when there is wind without once flapping its wings.

The wing, as a rule, is more flattened in the insect than in the bat and bird. It is, moreover, driven at a higher speed, those animals which fly the quickest having for the most part the flattest wings. The dragon fly furnishes a good example.

The greater the concavity of the wing, the greater the elevating power; the flatter the wing, the greater the propelling power.

The wings in living animals are thoroughly under control both during the down and up strokes; the wing, consequently, is not simply an elastic apparatus, which derives the movements of its separate parts from the air; on the contrary, it directs and controls the air in such a manner as to extract the maximum of support and propulsion from it.

The wings of bats and birds are moved by direct muscular action in combination with certain *elastic* ligaments, and the same holds true of the dragon fly and some other insects. The elasticity of the wing and the resiliency and reaction of the air, however, assist the muscles and ligaments.

The great speed attained by the tip and body of the wing is due to the fact that the wing is articulated or jointed at its root, any movement communicated at the root being quickened in proportion to the distance from the root. In other words, a compara-

tively slow movement communicated to the root of the wing is at once converted into a very rapid one at the tip.

If an artificial wing be constructed in strict accordance with any of the natural wings (insect, bat, or bird), and applied by a sculling figure-of-8 movement to the air, it will be found to supply a steady buoying and propelling power, similar in all respects to that supplied by the living wing.

In order to secure this result, the artificial wing should be concavo-convex, and slightly twisted upon itself, *i.e.*, it should be finely arched in every direction. It should be mobile as well as elastic,* and be applied to the air at different angles and at different degrees of speed, in such a manner that the wing and air may be active and passive by turns.

The *artificial wing*, like the natural one, must be more or less triangular in shape. It must taper from the root towards the tip, and from the anterior margin in the direction of the posterior margin. It should be capable of change of form, and elastic throughout, the flexibility being greatest at the tip and posterior margin of the wing, and least at the root and along the anterior margin. It must move in all its parts at different periods of time, as in this way the air is alternately seized and dismissed, dead points avoided, and a continuous reciprocating movement secured. In producing a continuous vibration of the artificial wing, much assistance is obtained by employing a ball-and-socket joint at its root, with a system of elastic springs of different strengths. The principal springs should be arranged at right angles to each other, the superior and posterior springs being stronger than the inferior and anterior ones. Oblique springs may be added, and the whole, because of their different strengths and their peculiar directions and insertions, can be made to give the wing any amount of torsion in the direction of its length during every portion of either the up or down stroke. The muscles and elastic ligaments of insects, bats, and birds, perform a similar function. A ball-and-socket joint, or what is equivalent thereto, is necessary at the root of the wing,

* Borelli (1668), Durkheim, and Marey state that an artificial wing should be composed of a *rigid rod* in front and a flexible sail behind, but experiment has convinced the author that no part of the wing should be absolutely rigid.

because the pinion should be free to move in an upward, downward, forward, and backward direction. It should also be able to rotate around its anterior margin to the extent of nearly a quarter of a turn. All the movements referred to are derived in the author's models from a *direct piston action*, from the reaction of the air, the elasticity of the wings and springs, and the weight of the machine bearing the wings. They are restrained and directed by the gearing apparatus extending between the piston and the wings, but more especially by the different lengths, strengths, and directions of the elastic springs themselves. The piston is made to descend with a very violent hammer-like motion at the beginning of the down stroke, the movement being gradually slowed as the wing descends to a certain point, at which the movement is reversed and the piston ascends more slowly, its ascent being occasioned for the most part by the reaction of the air, the elasticity of the wing and of the springs at its root, and by the descent of the engine propelling the wings. The driving power, the weight of the apparatus, the recoil of the air, and the elasticity of the wings and springs are thus made to act in concert, the different forces being active and passive at intervals, and no two forces acting together at precisely the same instant of time.

If a longitudinal section of a bamboo cane, 10 feet in length and half-an-inch in breadth, be taken by the extremity and made to vibrate, it will be found that a wavy serpentine motion is produced in it, the waves being greatest when the vibration is slow, and least when it is rapid. It will further be found that, at the extremity of the section where the impulse is communicated, there is a steady reciprocating movement devoid of dead points. The continuous movement in question is no doubt due to the fact that the different portions of the reed reverse at different periods, the undulations induced in the reed being to an interrupted or vibratory movement very much what the continued play of a fly-wheel is to a rotatory motion.

If a similar reed has added to it at its outer or distal half tapering rods of whalebone radiating in an outward and backward direction to the extent of a foot or so, and the whalebone and the reed be covered with a thin sheet of india-rubber, an artificial wing resembling the natural one in all its essential properties is at once

produced.* Thus if the wing be made to vibrate at its root, *a double wave is produced*, the one wave running in the direction of the length of the wing, the other in the direction of its breadth. The wing further *twists* and *untwists* figure-of-8 fashion during the down and up strokes. There is, moreover, a continuous play of the wing, the down stroke gliding into the up one, and *vice versa*, by a system of continuous and opposite curves, which clearly shows that the down and up strokes are parts of one whole, and that neither is perfect without the other. This form of wing is endowed with the very remarkable property that it will fly in any direction, demonstrating more or less conclusively that flight is essentially a *progressive wave movement*. Thus if the anterior or thick margin of the wing be directed upwards, and the angle which the under surface of the wing makes with the horizon be something less than 45 degrees, the wing will, when made to vibrate, fly with an undulatory motion *in an upward direction*, like a pigeon to its dove-cot. If the under surface of the wing make no angle, or a very small angle with the horizon, it will dart forward in a series of curves *in a horizontal direction*, like a crow in rapid horizontal flight. If the angle made by the under surface of the wing be reversed, so that the anterior or thick margin of the wing be directed downwards, the wing will describe a wave track and fly *downwards*, as a sparrow from the top of a house or tree. In all those movements *progression* is a necessity; the movements are continuous gliding *forward movements*; there is no halt or pause between the strokes, and if the angle which the wing makes with the horizon be sufficiently great, the amount of steady, *tractile*, and *buoying power* developed is truly astonishing. This form of wing elevates and propels both during the down and up strokes, and its working is accompanied with little or no slip. Its movements may be regarded as the literal realisation of the figure-of-8 hypothesis of flight.

* The author has made a great variety of artificial wings. Of these some are in one piece, with a continuous covering; others in a single piece, with the cover broken up into a large number of small valves; others in several pieces, with a continuous covering, and others jointed, with the cover broken up into a number of valvular segments. In all cases the frames of the wings are composed of *elastic material*, such as steel tubes, bamboo and other canes, osier twigs, whalebone, gutta percha, &c., &c.; the covers of the wings are made of india-rubber cloth, tracing cloth, argentine, linen, silk, &c., &c.; the springs of the wings of steel, caoutchouc, &c., &c.

If the artificial wing be in one piece, it ought to be made to vibrate in a more or less horizontal direction; if in several pieces, it should be worked in a more or less vertical direction, as the wing in this case acts alternately as a short and long lever, in virtue of its closing and opening during the up and down strokes, the acting area of the wing being greatly reduced during the up stroke, and greatly increased during the down one.

If a properly constructed artificial wing be made to vibrate in a vertical direction, it invariably darts *downwards and forwards in a curve* during the down stroke, and *upwards and forwards* in a *similar* but *opposite curve* during the up stroke, the two curves running into each other to form a progressive, continuous, *wave track.*

If the wing be made to vibrate from side to side in a more or less horizontal direction, it rises zig-zag fashion by a series of looped curve movements, each pass of the wing being on a higher level than that which preceded it. Whether the wing be moved vertically or horizontally, it invariably twists and untwists during its action. In twisting and untwisting, it developes figure-of-8 curves, not only along its anterior and posterior margins, but throughout its entire length and breadth.

The figure-of-8 vertical movement may be converted into the figure-of-8 horizontal movement by a slight rotation of the wing on its long axis, or by a tilt of the body or frame bearing the wing. It is in this way that the wing may act either as an elevator and propeller, or merely as an elevator. Thus it is not uncommon to see an insect elevate itself by a horizontal screwing figure-of-8 movement, and then, suddenly changing the direction of the stroke of the wing and of the body, dart forward in a nearly horizontal direction.

The artificial wing, like the true one, attacks the air at a great variety of angles during the down and up strokes. Thus during the down stroke the angles which the wing makes with the horizon are increased, whereas during the up stroke they are diminished.

The angles made by the different portions of the artificial wing vary as in the living wing, the angles made by the parts nearest the root being greater than those nearer the tip. This is occasioned by the manner in which the artificial wing *twists* and *untwists* during its action, the torsion in question being due to the

elastic properties of the wing and the resistance which it experiences from the air, as well as to the fact that the tip and posterior part of the wing travel at a much higher speed than the root and anterior part. The small angle made by the tip, as compared with the root of the wing, equalises its action, a large angle urged at a low speed giving the same amount of buoyancy and propelling power as a smaller angle urged at a higher speed.

The artificial wing, because of its elasticity and by the aid of certain springs, can be made to slow and reverse of its own accord at the end of the down and up strokes in precisely the same manner as the natural wing. It can likewise be made to change its course without halt or dead point, so as to give continuity of motion and continued buoyancy.

If the artificial wing be moved figure-of-8 fashion in a more or less horizontal direction, it can be made to create and utilise its own currents, the stroke from right to left producing the currents on which the wing rises in its passage from left to right, and the reverse. It can also be made to utilise and evade natural currents.

If the tip of a properly constructed artificial aërial wing be turned downwards, and the wing be made to move from side to side figure-of-8 fashion like the tail of a fish, it forms a very excellent aërial propeller.

The artificial wing, to be effective, must rotate about two separate axes, the one corresponding to its root (short axis), the other to its anterior margin (long axis).

If two artificial wings, similar to those described, be placed end to end, inclined at a certain upward angle, and made to revolve, they form a most powerful aërial screw. This form of screw is propelled with comparatively little force, and its working is attended with quite a nominal amount of slip.

The aërial screw here recommended *is elastic* and *capable of change of form* in all its parts, and so constructed that its angles vary to adapt themselves to the speed attained by the different portions of the blades at any given time. Thus the angles made by the blades are greatest when the speed at which the screw is driven is least, and *vice versa ;* the angles made by those portions of the blades which are nearest the axis of rotation being always greater than those made by the portions nearer the tips of the blades. This form of

aërial screw differs widely from the aerial screws at present in use, and from the screw propeller employed in navigation, inasmuch as it is moveable in all its parts, and adjusts itself to its work in such a manner as to secure the maximum of elevating and propelling power, with a minimum of slip. The screw propeller and aerial screws as at present employed are, on the contrary, *rigid and unyielding*, and possess no accommodating power. As a consequence, much propulsive power is sacrificed in slip.

If the blades of the aërial screw referred to be greatly diminished in size, and formed of carefully tapered, finely graduated steel plate, it operates with remarkable efficiency in water, the elasticity of the screw diminishing the slip, while it greatly augments the propelling power.

The following Gentlemen were admitted Fellows of the Society:—

Rev. Thomas M. Lindsay, M.A.
William Robertson Smith, M.A.
Stair Agnew, Esq.

Monday, 30th January 1871.

Professor KELLAND, Vice-President, in the Chair.

At the request of the Council, Dr J. Collingwood Bruce delivered an Address on "The Results of the More Recent Excavations on the Line of the Roman Wall in the North of England."

Nearly a century after Julius Cæsar had landed in this island the conquest of Britain was begun in earnest.

In the year 79 Agricola planted the Eagles of Rome on the banks of the Tyne, and during the next campaign carried his conquests as far as the Tay. Before he gave up his command, he had raised the Roman standard in the Orkney Islands.

When Rome planted her foot she usually planted it firmly, and thus she retained in her grasp all the best portions of the island for more than 300 years. Some of the *legions* which landed in the

time of Claudius remained in the island until the close of the Roman domination.

In the year 410, when Alaric and his Goths entered Rome, Honorius renounced all claim upon the allegiance of Britain.

As to the origin of the wall, when Agricola advanced against the Caledonians, he thought it necessary to use precautions against a rising amongst the conquered tribes whom he left behind him. He made good roads contemporaneously with his advance. As he moved along he drew the road with him. By this means his retreat was always secure and his supplies comparatively certain. It is believed that we owe to him the northern Watling Street and the Maiden Way, which run northwards parallel to each other at about twenty-five miles apart. For miles together both of these roads remain to this hour as the Romans left them. Another precaution adopted by Agricola was the planting of garrisons in well-selected situations. There were two parts of the island where these garrisons could be best placed, namely, where the influx of the sea brings the eastern and western coasts into near contiguity—between the Firths of Clyde and Forth, and between the Tyne and Solway. Here walls were afterwards built. The southern wall was not a mere fence. It was a line of military operation. In erecting it the Romans did not give up the country to the north of it, but by its means made it more thoroughly their own. A transverse road along it was a necessary adjunct. At the Northumberland Isthmus Watling Street and the Maiden Way went north and south; another road, which has been called the Stanegate, went from east to west.

Dr Bruce then enumerated some of the principal stations in the wall as amplified and finally completed by Hadrian, who made use of such of the pre-existing stations of Agricola as served his purpose.

The stationary camps on the Roman wall usually have four gateways, one in each end, and one in each side rampart. Each gateway consists of two portals divided by strong piers of masonry, with its own arch overhead. There is uniformly a guard chamber on each side of the gateway.

The wall, as erected by Hadrian, exists to this day in wonderful completeness. Except in places where towns have sprung up on

its site, there is scarcely a yard of its course from Wallsend to Bowness where traces of it are not to be found. Where the stone-works have disappeared the fosse or earthen ramparts generally show themselves.

The wall is really an important fortification, consisting of several parts. There is first the stone wall, with a deep and broad fosse on its northern margin; next, the vallum or earth wall, which at varying distances keeps to the south of the stone wall. Then between these was a well-made road. Lastly, there was a series of stationary camps, castles, and turrets, for the accommodation of the soldiery who garrisoned the structure.

The length of the great wall is said to be seventy-three and a-half miles. It is usually about eight feet thick, and in two places it now stands nine and a-half feet high. Its original elevation was much greater.

The stations were military cities, mostly attached to the wall. The largest of them contain an area of six acres, some of them only three. The stations are distant from one another at an average of about four miles. Their form is that of a parallelogram with the corners rounded. The first thing which the builders of the wall did was to build the station, when they felt that they could safely undertake the other parts of the fortification, running the wall right and left. The masonry of the gateways is peculiarly massive and strong. In some of them the joints are as close as ever, and the courses as true as they were 1700 years ago. As far as can be ascertained, every station had a double gateway opening northwards, as well as in other directions. The north gate of Borcovicus station (House-steads) must have been much used, for its threshold is deeply worn by the feet of passengers.

That the Romans did not give up to the enemy the country on the north side of the wall is shown by a circumstance that the garrison at the station of Borcovicus had an amphitheatre provided for their amusement on the north side of the wall, where the ground outside the wall was best suited for its formation. It was not unusual with the Romans to provide amusements for the soldiery even upon a campaign.

In crossing from sea to sea, the wall, about the centre of its course, comes near an upheaved mass of basalt. For about ten

miles it takes advantage of this circumstance, and swerving out of its direct course, seizes hill after hill, so as to present to the enemy not only the obstacle of its own height, but that of the ridge of which it is built. A similar and more striking one of the natural ground is seen at Peel Crag.

When the wall runs over precipitous ledges like this, the fosse on the north side of it is of course discontinued, but the moment it again descends into the valley it is renewed.

Dr Bruce's paper contained several other particulars illustrating the present condition of the wall, and showing the powerful and systematic organisation displayed in its construction as a means of commanding and keeping in subjection the adjacent country. It also contained references to the monuments and inscriptions found in the line of the wall, indicating in particular the prevalent religious feelings of the period, and in particular showing an infusion of Eastern ideas into the native mythology of the Romans.

The following Gentlemen were admitted Fellows of the Society :—

Charles Hayes Higgins, M.D.
Angus Macdonald, M.D., F.R.C.P.

Monday, 6*th February* 1871,

Dr CHRISTISON, President, in the Chair.

The following Communications were read :—

1. Note on two Species of Foraminifera, and on some Objects from the Nicobar Islands of great Ethnological interest. By T. C. Archer, Esq. Specimens were exhibited.

Mr Archer exhibited two interesting Foraminifers, one being *Saccammina Carteri*, which forms a large proportion of the Carboniferous limestone at Elfhills, Northumberland; the other, a gigantic species of the Arenaceous group brought from Persia by the late Mr Loftus, and named after him, *Loftusia persica*. The latter specimen was that to which Mr Archer especially called the attention of the

Society, as it was similar to a class of fossils which had previously been found in the Upper Greensand formation in England, and believed to be sponges. However, the whole history of these monsters of their Order has been so well worked out in the admirable monograph of Dr Carpenter and Mr H. B. Brady, that their proper character is now thoroughly known.

Mr Archer also exhibited some objects of great Ethnological interest from the Nicobar Islands.

The following is the Memorandum accompanying the Wooden Figures obtained by Captain Edge, R.N., commander of H. M. S. "Satellite," from the Nicobars, in July 1867.

Reports having reached the authorities at Singapore that several vessels had, from time to time, been attacked by the savages upon these islands, and their crews barbarously murdered, it was determined to despatch an expedition to that spot; and accordingly, in July 1867, H. M. ship "Wasp," Captain Bedingfield, R.N., and H. M. ship "Satellite," Captain Edge, R.N., proceeded thence. The savages fled on the approach of the vessels of war, and upon landing at Enounga, one of the largest of the villages, Captain Edge discovered these figures in their huts, and upon his return to Singapore he gave them to Major M'Nair of the Royal Artillery for presentation to a museum.

The photographs are those of three of the savages who were captured, and of a little girl of seven years of age, who was rescued from their hands and brought to Singapore.

List of Wooden Figures from the Nicobar Islands, procured by Captain Edge, R.N., and presented to the Edinburgh Museum of Science and Art, by James M'Kenzie, master of the ship "Shree Singapora."

1. Large figure of a woman.
2. Male idol.
3. Figure of a native male in European style.
4. Do. do. (smaller size).
5. Figurehead of a native female.

6 & 7. Two small figures.

8. Figure of an animal.

These specimens were exhibited to the Ethnological Society in London at the beginning of last year.

After all that has been read of the complete absence of any kind of Art amonst the savages of these islands and the neighbouring Andamans, one is irresistibly led to think that these objects are not the works of the natives, but have been produced by some debased European or other captive.

2. Certain Phenomena applied in Solution of Difficulties connected with the Theory of Vision. By R. S. Wyld, Esq.

The theory of vision has been the subject of much more scientific study than that of any of our other senses, but notwithstanding this, the subject is still encumbered with some difficulties and contradictions, the solution of which is essential to our having a true and complete theory. Such are the questions,—first,—regarding single and double vision, as depending on the excitement of corresponding, or, as they are generally called, identical points of the retinæ; second,—the question whether perception is in the retinæ or in the brain; and lastly, the question regarding the decussation and ultimate course of the fibres of the optic nerves.

Regarding the subject of *single vision* with two eyes, there has frequently been exhibited a great amount of misunderstanding; since the discovery of the stereoscope, however, the nature of what has commonly, though not with strict propriety, been called single vision, has become much better understood. The truth is, there is no such thing as single vision when two eyes are in use, and a very little attention will make it clear how the case stands. Take two shillings of like appearance, and place them correctly and with the same sides up, in the different compartments of the stereoscope, but so far apart that they do not appear to coalesce. In this position they are distinctly seen by each eye as two separate objects. Cause the coins next gradually to approach till they seem to coalesce or unite into one—we say seem, for there is no true visual union. Even when they seem to unite, there are still two impressions made—one on each retina—and a corresponding impulse is from each of these membranes sent to the brain and to the mind, though from the close resemblance of the two impressions it may be impossible to distinguish the one from the other.

To prove that there are two mental impressions, let us reverse one of the coins. When this is done, we have no longer the impression of one coin, but of two coins occupying the same place. Both are visible, and they appear as if the one were visible through the other. While we steadily regard this anomalous presentation, the eye and the judgment become alike puzzled by it, and an effort is made to reduce the phenomenon to a normal and intelligible object of vision; a succession of transformations is the result of the joint action of the mind, and of the disturbed nervous centres which ensues; at one moment we see one coin, and then, suddenly, it disappears, and the other takes its place; then we see both coins at once, or a part of each perhaps becomes alone visible. In ordinary vision, then, we must conclude that objects make an equal impression on the identical points of each retina, though we are not intellectually conscious of the fact of duality; and the question thus arises, If there are two retinal impressions, how do we account for the two appearing as if superimposed the one on the top of the other? The eyes are set apart in the head, and the supposed sensory ganglia at the base of the brain, the *corpora geniculata*, the *corpora quadrigemina*, and the optic thalami, are all in duplicate: and the cerebral hemispheres divide the head in two equal sections. How, then, are we to account for the two visual images being united? It has been very generally assumed that the mind combines the two impressions, as it were, into one. This is the opinion of Professor Wheatstone and Dr Carpenter, and it was for many years my opinion; but the phenomena about to be alluded to convinced me that I was wrong, and that there exists a physical cause for the union of the two images; and to prove this is the main purpose of the paper.

When we take two strips of white card-board about an inch broad, and insert one at each side of the stereoscope, we find that each strip is distinctly seen by each eye; but when we cause them gradually to approach till the two ends appear to overlap say an inch or more, the effect is singular. Where the strips seem overlapping, the brightness is observed instantly to become very much increased: so much so, indeed, that when we fix the attention on the quadrangular part formed by the overlapping ends, all the rest of the strips become invisible, and the overlapping parts alone

remain distinct objects of vision. It may however be mentioned, by the way, that either of the cards may be recalled to sight by the simple act of moving it two or three times backwards and forwards, and thus exciting the nerve and arousing the attention; but this in no degree impairs the superior brightness of the overlapping parts.

Such are the facts, but what is the cause of the increased brightness where the cards appear to overlap, and what is the cause of the apparent overlapping where corresponding points of the retinæ are excited by objects in reality apart? I am not aware of any writer having distinctly laid before us a specific physical cause accounting for these several phenomena. It appears to me that they clearly point to an anatomical cause.

A great many writers have attributed single vision to habit. Dr Smith in his optics attributes single vision to this cause. Dr Carpenter also seems to take this view. He says ("Physiology," p. 705), "A condition of single vision seems to be that the two images of the object should fall on parts of the retinæ accustomed to act in concert, and habit appears to be the chief means by which this conformity is produced." Dr Reid, in his "Inquiry into the Human Mind," states that he has devoted thirty years to the study of the subject, and he accepts it as a mystery which cannot be explained. Sir Wm. Hamilton attempts no explanation. Neither does Sir D. Brewster in his famous controversy with Professor Wheatstone attempt any explanation. Buffon thinks we first see objects double and inverted, and that we correct this judgment by experience. Blanville, Gassendus, Porta, Tacquet, and Gall, maintain that we see with only one eye at a time.

Perhaps the majority of writers have looked no deeper than the surface of the retina, and have been content to state the phenomena as depending on an inscrutable property of that sensitive membrane, or simply as a law of our being: even as they, with quite as little ingenuity, and with less excuse, attribute our sense of visual direction to an inscrutable property of the retina. Some anatomists have, however, supposed that the decussation of the optic nerves might explain the phenomena. Dr Wollaston, from a peculiar occasional disorder in his vision, suggested that there was a crossing of the fibres from the *inner parts* of either retina to the

ganglion on the opposite side of the head, while the fibres on the *outer* side of each eye went to the ganglion on their own side of the head. This explanation evidently implies that the retinæ are optically divided in two halves, and that the images of objects falling on the centres of the retinæ are similarly divided, one half of every object being represented on the right side of the head, and the other half on the left; and that objects whose images fall on the one side of the retinæ are represented only on the lobe on that side of the head. This is surely extremely improbable.

Newton, in his optics, throws out a query (query 15th at the end of Second Book), suggesting that the species or picture of the objects seen with both eyes may be united in the commissure of the optic nerves, the fibres of the right side of both nerves uniting there, and, after union, going thence into the brain on the right side of the head, and the fibres on the left side of both nerves, after union in the commissure, going into the brain on the left side of the head, and the two meeting in the brain in such a way that the fibres make but one entire species or picture. The writer had not seen Newton's query till after his paper was submitted to the Council, but he considers that Newton's is the most advanced position which has up to the present times been taken on the subject. It is evident, however, that Newton had never very carefully reduced his idea to form, nor had he then the means which we now possess of testing its correctness; and it was doubtless owing to this circumstance that the idea, instead of being followed up and corrected in its details, was allowed to fall out of sight, and failed to gain the attention of optical writers.

Whether there is or is not a crossing of the true visual or optic nerves in man and the higher mammalia seems yet to be an unsettled point, though the opinion is gaining ground that there is a crossing of the inner fibres. It is always asked if there is no crossing of fibres, why are the optic nerves brought into connection? The question, as an argument in favour of the crossing, is, however, robbed of half its force, when we consider that the apparent union of the commissure may not be for a transfer of the true nerves of vision, but for effecting a union of the nerves essential for the nutrition of the retinæ, and of those nerves whose function it is to secure equality and unity of action in the reflex opera-

tions which regulate the expansion and contraction of the iris of the eyes.

I do not believe in any partial crossing of the true visual nerve-fibres. The fact, however, of an entire crossing, or of no crossing at all, in no ways affects my theory, which I shall now, after a few necessary words of explanation regarding the functions of the retina, proceed to explain.

The central point of the retina, the *fovea centralis*, is distinguished from the rest of the retina by its peculiar anatomical structure. It is also distinguished by its superior discriminating powers. It is the only part of the retina which takes minute cognisance of the forms of objects. We may satisfy ourselves of this by fixing the eyes on any word in a printed book held at the usual reading distance. While the eyes remain fixed on the middle of any word of, say six or seven letters, most persons will find that they are quite unable to perceive a single letter of the adjoining word. This proves how limited is the area of distinct vision on the retinæ.

When we fix the eyes on any distinct object in an extended landscape we turn the axis of each eye to the object especially examined, and the images of it fall on the *foveæ centrales*, and appear single. All the other objects in the landscape are mapped at the same time around these central points, on corresponding parts of each retina, *i.e.*, on parts which are correspondent in distance and direction, from the *foveæ centrales;* and these objects also, so far as we can see them, appear single. The remarkable circumstance, however, is, that the slightest shift or displacement of the axis of one of the eyes, and of the image on it, disorders correct vision, and produces the perception of a duplicate impression of the landscape. This circumstance has led authors very generally to the conclusion, as I have said, that either habit, or some inscrutable law of the retinæ, causes *single vision* when corresponding parts of that organ are impressed, and *double vision* when non-corresponding parts of the two retinæ are acted on. The writer maintains that these phenomena, and also the phenomenon of increased brightness obtained by the use of both eyes, can only be explained on the assumption or theory, that the retinal impulses of both eyes are united in a central cerebral sensorium. He, therefore, suggests

that the true optic or visual nerve-fibres from the retinæ cross at the optic commissure, that they are continued through the optic tracts, and sweep inwards to the *corpus quadrigeminum;* that those from the left eye enter that cerebral lobe at the right side, and spread across and forward in it in the form of an inverted cone; while the nerve-fibres of the right eye enter the same lobe at the left side, and spread in a like manner across it from left to right. The fibres from each eye thus cross each other in this lobe, which, from being an important central ganglion, and most intimately connected with the fibres from the optic nerves, the writer suggests as the probable sensorium in vision. The effect of this simple arrangement is, that the *corresponding nerve-fibres* from each retina are brought into juxtaposition, fibre to fibre; and in natural vision the sensorium thus becomes the organ in which the nervous impulses which come from the two eyes are united and grouped in the form they occupy on the retinæ.

When, then, in the experiment before-mentioned we advance the two strips of card-board but a short way at each side of the stereoscope, their images are found on the inner parts of each retina, and the ends of the strips are seen as two separate objects, because their images are thrown on non-identical portions of the retinæ, and different parts of the sensorium are accordingly impressed. When, again, the strips are advanced a little further, till the images begin to cross the centre of the retina of each eye, the spectator immediately sees the ends to overlap, and at the same time to acquire additional brightness. This evidently arises from the corresponding parts of each retina being impressed, and the two similar impulses being transmitted to that portion of the sensorium with which these parts of the retinæ are in connection,—each nerve-fibre from the one eye bringing its impulse into juxtaposition with the corresponding impulse from the other eye. And thus we account at once, for the increased brightness, and the apparent superposition of the images of external objects. A diagram at a glance shows how these are the necessary results of the arrangement of the nerve-fibres which we have suggested.

That the nerve-fibres coming from each eye are not *united* or *fused* in the sensorium, but merely brought into juxtaposition, is a fact also proved by the following experiment with coloured strips.

When we introduce a blue strip at the one side of the stereoscope, and a red or yellow one at the other side, till they appear to overlap or unite into one object, the result is increased brightness where they overlap; but there is no blending of the colours so as to produce purple or green. The one coloured strip, as in the experiment with the coins, shines through the other; or at one time the colours are alternately visible, at another time one-half of each coloured end only is visible, and occasionally spots of the one are seen to shine through the ground colour of the other, thus establishing the important fact or law, that though the combination of different colours, external to the living organism, produces the effect of an intermediate colour, yet the impulse of different colours on separate retinæ can not be so combined by the mind, but the impulse peculiar to each colour is conveyed by the nerve receiving it to the sensorium unchanged, and excites in the mind its own characteristic sensation. The increased intensity where the adjoining nerve-fibres in the sensorium are all in action I attribute to the well-known law of irradiation, or *lateral* expansion of nervous action, which exists among neighbouring nerve-fibres when powerfully excited

The arrangement of the fibres above suggested explains—

1*st*. The nature and cause of the peculiar action of the identical retinal points.

2*d*. The physical cause of single and double vision.

3*d*. The reason why we have increased brightness by the use of both eyes, whether in ordinary vision or when using the stereoscope.

4*th*. The several phenomena force us to the conclusion that visual sensation is not in the retinæ, but in a common cerebral sensorium.

3. Additional Note on the Motion of a Heavy Body along the Circumference of a Circle. By E. Sang, Esq.

Abstract.

In the course of physical inquiries we meet with many problems having the appearance of great simplicity, and yet presenting to the analyst difficulties of the highest order. The law of the motion of a heavy body along the circumference of a circle is one of these.

One particular case of this motion, viz., the case of the swinging of a clock-pendulum, is of paramount importance, and has been investigated with very great care. In this case our attention is directed principally to the computation of the time of an entire oscillation, since it is this which determines the beating of the clock. In the paper to which this note is an addition (Vol. xxiv Trans.), a very rapid method of computing this total time is given. My object is now to supply the deficiency in that paper, and to show how the time of describing any given portion of the whole arc may be computed.

The general question may be stated thus:—A heavy body is projected with a known velocity along the circumference of a circle, and we are required to compute the time in which it will reach any indicated position, as also its place at any prescribed time.

No practicable solution of either of these problems has hitherto been given, with the exception of the case already mentioned. This note contains a simple and complete solution of both problems.

If a heavy body be projected from the lowest point of a circle along the circumference with a velocity less than that due to a fall from the highest point, its motion becomes slower as it ascends, and its speed is entirely exhausted at some point in the semicircumference; from that point it returns to the bottom of the curve, passes to the other side, and so oscillates. But, if the initial velocity be greater than what is due to a fall along the diameter, the body passes the zenith point, and circulates round and round the circumference with an unequable motion. And if the velocity be just sufficient to carry the body to the zenith point, it rests there, and the motion ceases. Now, while the investigation of the oscillatory and of the continuous motion is difficult, that of the limit between the two is easy.

If the body move away from N with a velocity due to a fall through the distance ZN, it will have, when it reaches the point A, a velocity due to a fall through ZG. But the distance through which a weight falls freely is proportional to the square of its acquired velocity, and ZG is proportional to the square of ZA; wherefore the velocity at the point A must be proportional to the chord ZA; that is to say, the rate of increase of the angle NZA is

proportional to its own cosine; or, writing A for this angle, we have

$$dA \propto \cos A \,.\, dt, \quad dt \propto \sec A \,.\, dA$$

and, therefore, the time occupied in passing over some fixed minute portion of the arc at A is proportional to the secant of the angle NZA.

In Mercator's Projection of the Sphere, the differences of the meridional parts are proportional to the secants of the latitudes, wherefore the time of describing the arc NA must be proportional to the meridional part corresponding to the angle NZA, that is, must be proportional to the logarithmic tangent of $45° + \frac{1}{2}A$. Measure off then some distance ZE horizontally to represent the linear unit, and bisect the angle AZE by the line ZT meeting the plumb-line from E in T, the time of passing along NA is proportional to the logarithm of ET, or rather to the logarithm of the ratio of ET to EZ. Hence, when the angle is given we can readily compute the time, or when the time is given we can as readily compute the angle; and thus for this particular case the problem is completely resolved.

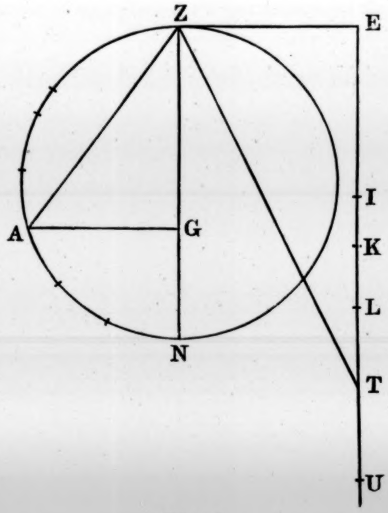


Fig. 1.

Making EI equal to EZ, if we make a series of continued proportionals EI, EK, EL, ET, EU, &c., and, joining Z with the several points, make angles doubles of EIK, EIL, &c., we shall obtain the positions of the moving body after equal intervals of time. The time of its reaching Z is thus infinite.

The relation of the continuous to the reciprocating motion may be exhibited by a simple contrivance. Let two straight rods AC, CB be jointed at the point C, and let the two ends A, B be connected by a straight line, say an elastic thread.

If the rods be turned so as to lessen the angle ACB, the angles

at A and B will increase. If the motion be sufficiently continued, the greater angle A will become a right angle, and then B will have reached its maximum. Should the motion be still further continued, A becomes obtuse and B decreases; till, when the rods have entirely closed, A becomes 180° and B becomes zero. Continuing the angular motion, A becomes a reverse angle, and B appears on the opposite side of AB. Thus the alternate increase and decrease of the smaller angle B resembles the changes of the angle NZA (fig. 1), when the motion is oscillatory. And at the same time the continual development of the angle at B

Fig. 2.

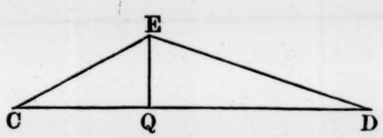


Fig. 3.

resembles the change of NZA when the heavy body over-passes the zenith point. The resemblance is a close one, for if we suppose CAB to increase with a velocity proportional to the distance PB, intercepted by the perpendicular CP, its variations are then exactly analogous to those of the angle NZA, when a heavy body revolving in a circle whose diameter is proportional to AC, has its velocity at the lower point equal to that obtained by falling through a distance proportional to CB. And similarly the variations of the smaller angle B are analogous to the oscillations of a heavy body in another circle, the greatest height being to the whole diameter in the ratio of AC to CB.

When AC is very small in comparison with CB, the maximum angle B is also small; that is to say, the arrangement represents an oscillation in a small arc; but when the two rods are nearly of equal lengths, as in the case of CE, ED (fig. 3), the maximum value of D approaches to a right angle, and the arrangement represents an oscillation extending to nearly the whole circum-

ference. If the trigon were isosceles, the representation would be that of the motion which we have already investigated.

If the angle A vary with a velocity proportional to PB, and B with a velocity proportional to AP, the exterior angle at C must have the rate of its variation proportional to AB. Now, if we make DCE (fig. 3), equal to half the sum of CAB and ABC, CE a mean proportional between AC and CB, and then inflect ED equal to half the sum of the same lines, the perpendicular EQ intercepts QD just half of AB. Thus QD is proportional to the rate of increase of ECD, and consequently CQ to the rate of change of CDE. Thus the synchronous variations of the trigons ACB and CED would represent four connected cases, two of oscillation and two of revolution in a circle.

Now, the ratio of CE to ED is much nearer to one of equality than is the ratio of AC to CB; and if we were to proceed again in the same way, we should obtain a trigon still more nearly isosceles; and, after a very few operations of this kind, we shall obtain a trigon sensibly isosceles. That is to say, we shall have referred the oscillation in a given arc to the motion in just the whole circumference. So, seeing that the motion in this last case has been completely investigated, we have a complete solution of the general problem; the necessary calculations being of remarkable simplicity.

4. On the Capture of a Sperm Whale on the Coast of Argyleshire, with a Notice of other Specimens caught on the Coast of Scotland. By Professor Turner.

In the autumn of last year, whilst spending a few days in the neighbourhood of Oban, I visited Dunstaffnage, and in the court-yard of the Castle saw the two halves of the lower jaw-bone of a sperm-whale. On inquiry, I learned that they were the relics of a whale captured some years ago in the neighbouring sea. From some of the older inhabitants of Oban I gleaned some particulars respecting this animal; and as no record of its capture has as yet found a place in zoological literature, I am induced, as the sperm-whale so very seldom visits our shores, to communicate a brief notice to the Society.

In the month of May 1829 a large whale was seen spouting in

the Sound between Lismore, Mull, and the mainland. The fishermen were at first afraid to approach it, but as, after a few days, the animal became less active in its movements, they sallied forth in boats, and inflicted severe wounds with harpoons and other weapons. The animal was then secured, and towed ashore in Dunstaffnage Bay, close to the ruins of the Castle. It was said to have been about 60 feet long, and possessed a very bulky head, with a square snout. It was at once seen to be very different in its form and appearance from the large whales which usually visit our shores; but it was not until an oily fluid, which flowed out of a wound near the snout, and congealed on the surface of the water, was recognised to be spermaceti, that the character and value of the animal was determined. A considerable quantity of spermaceti was obtained from the great cavity in the head, and the blubber yielded a large amount of oil. I could learn nothing definite as to the sex.

The lower jaw was preserved as a relic in Dunstaffnage Castle, and, in the garden of one of the hotels in Oban, I met with a caudal vertebra, which was said to have belonged to this animal.

When I saw the jaw it was much injured. Not only were all the teeth lost, but the symphysial ends of both halves were broken off, and the expanded articular portion of the right half sawn off and removed. It is to be feared, if some care be not taken to preserve the fragments which remain, that in a few years all trace of this rare and interesting specimen will have disappeared.

From the left mandible some measurements were obtained which may give an approximation to the dimensions of the bone. The length was 149 inches; but as the anterior end was absent—as, indeed, only the sockets of sixteen teeth remained—this measurement falls several inches short of the original length of the bone. The articular end was expanded, and possessed a vertical diameter of 22 inches. On its inner face was the very large opening of the dental canal. Close to the junction of the articular and dentary parts of the mandible was a well-marked constriction, where the bone measured only 8 inches in breadth. The breadth of the alveolar edge of the jaw, about its middle, was $4\frac{1}{2}$ inches. In its general form the mandible was broad and thin at its articular part, then constricted, beyond which it dilated, and then gradually tapered away to the anterior extremity.

The first instance on record of the stranding of a sperm-whale on the Scottish coasts is the specimen described in the "Phalainologia Nova," by Sir R. Sibbald, which came ashore at Lime Kilns, on the north side of the Forth, in February 1689. It was a male, 52 feet long, and had 42 teeth in the lower jaw. Several portions of this animal were preserved by Sibbald in his museum, and formed a part of the collection which was presented by him* to the University of Edinburgh.

In the copy of the "Phalainologia Nova," in the library of the Royal College of Physicians of this city, a manuscript letter has been inserted, in which an account is given of the stranding of another sperm whale in the Forth. The manuscript is entitled "Part of a Letter from Mr James Paterson, Keeper of the Balfourean Museum at Edinburgh, to Mr Edward Lhwyd, Keeper of the Ashmolean Museum at Oxford. Edinburgh, July 22, 1701." Penes E. W.†

"There was lately a pretty big whale came in at Crawmond. It had no whalebone, and teeth only in the lower jaw, which, according to Sir R. Sibbald, is the characteristick of yt kind which has ye sperma cete. You have ys figured in Jonston, tab. 42 of his Fishes.‡ Diverse of our physicians were present at ye opening

* Auctarium Musæi Balfouriani e Musæo Sibbaldiano: sive Enumeratio et Descriptio Rerum Rariorum, tàm Naturalium, quam Artificialium, tàm Domesticarum quam Exoticarum: quas Robertus Sibbaldus, M.D. Eques Auratus, Academiæ Edinburgenæ donavit. Edinburgi, impressum per Academiæ Typographum, Sumptibus Academiæ, 1697. In this catalogue, under the head "De Piscibus Viviparis Raribus," the following specimens obtained from this sperm whale are referred to:—A tooth, the crystalline humour of the eye, a fragment of the flesh and skin, and a specimen of spermaceti from the head. "The Sperma Ceti was lodged most of it within the skull of it, which was of a prodigious bigness."

† Mr Small, the Librarian to the University and to the College of Physicians, informs me that the initials "E. W." are in all probability those of Dr Edward Wright of Kersie, who became a Fellow of the College in 1753. His valuable library of works on natural history, of which the copy of the "Phalainologia Nova," above referred to, formed a part, was presented, in 1761, to the College by Alexander Gibson Wright, Esq. of Cliftonhall.

‡ The "Historia Naturalis," by Joannes Jonstonus, M.D., was published at Amsterdam in 1657. Book v. De piscibus et cetis, contains a folio plate, tab. 42, on which is represented a great whale, 60 feet long, lying on its right side, and presenting its abdomen, with a large pendulous penis, to the observer. From the form of the head and the shape of the lower jaw it is

of ye head, where they got 2 barrels of sperma cete: This filled up the whole cranium; they could find no other thing they could call ye brain, if it were not a friable cineritious-like substance, which seemed very improbable. They found ys sperma, not only in ye head and spina dorsi, but (which perhaps has not been hitherto observed) dispersed through ye whole body; in ye glands, whence they prest it out in considerable quantities. The chyrurgions spoke of buying the skeleton; but I don't know how it came, ye owners disposed of all another way, so yt neither they nor we got anything of it. Dr Sibbald got a tooth. He has made a description of it, and says he has materials for a 2nd part of his 'Phalainologia.' Our whale was a male: the penis appeared near 7 feet without ye body. The whole length of the creature was near 52 feet, and ye circumference of ye biggest part of it about 30. The nether jaw was only 3 foot ½ about, and had 48 teeth in it. The upper jaw had sockets lined with cartilages to receive 'em."

Dr Wright has also inserted into the same copy of the "Phalainologia Nova" a plate containing six figures, which are marked as follows:—Fig. 1. Balæna fœmina, pinnis et cauda sinuatis; fig. 2. Balæna Macrocephala in faciem obversa, ut dorsum appareat; fig. 3. Eadem in latus decumbens; fig. 4. Delphinus; fig. 5. Phocœna; fig. 6. Pediculus Ceti Bocconi.

In explanation of this plate, Dr Wright states—"This plate I found in a book of original drawings of Sir Robert Sibbald's, which I met with accidentally some years ago. All the explanation I could make out is as follows:—Fig. 1. The original drawing is marked in Sir Robert Sibbald's own hand, 'A Whale cast in at Resyth Castle.' Figs. 2, 3, marked in Sir Robert's hand, 'A Sperma Ceti Whale;' and in another hand, 'Whaile at Monyfeith, Feb. 23, 1703—(fig. 2) backe, to represent the taill; (fig. 3) side; but it did lay halfe upon its side that one Ey & a litle of the bellie was

obviously a sperm whale. The drawing has clearly been made from the animal as it lay on the beach, as the coast line, and numerous figures of persons, either gazing at the whale or on their way to see it, are carefully given. The whole plate has an air of truth and nature which contrasts favourably with the imaginary figures of dragons, mermaids, basilisks, griffins, and unicorns represented in other parts of the work.

sanded. 57 foots long and 56 round, tooth under, & all the skin blackish blew, werie smooth, and as thick as a bull's, & all white fat within & nixt the skin.' "

Figures 2 and 3 are very fair representations of the back and left side of a male sperm whale, and the plate was in all probability prepared for the second part of his "Phalainologia," which does not seem, however, to have been published.

In the year 1756 a sperm whale, 63 feet long, is said to have been stranded on the west coast of Ross-shire.*

In the year 1769 a third specimen was seen in the Forth. It ran ashore on Cramond Island, on December 22, and was there killed. It was described and figured by Mr James Robertson, of Edinburgh, in the "Philosophical Transactions."† This animal was a male, and measured 54 feet in length, the greatest circumference being 30 feet.

In the Statistical Account of Scotland, vol. v., 1793, it is stated in the account of Unst, in Shetland, that "the spermaceti whale sometimes wanders to this coast, and is here entangled and taken." The Rev. George Low, in his "Fauna Orcadensis," 1813, says that the sperm whale "is often drove ashore about the Orkneys, and sometimes caught. One, about 50 feet long, was caught in Hoy Sound, some years ago, from which was extracted a vast quantity of spermaceti; as also another, which drove ashore in Hoy."

The most recent specimen, also a male, of this animal was washed ashore, in a much decomposed state, in July 1863, near Thurso. The skeleton was presented to the British Museum, and formed a part of the material from which Professor Flower has drawn up his admirable account of the osteology of the sperm whale.

This whale, in the tropical or semi-tropical seas, which more especially are its proper habitat, moves about, as a general rule, in large herds or "schools." The eight well-authenticated specimens which have now been captured on the Scottish coasts have been solitary animals, which have wandered northwards, perhaps, in the track of the Gulf Stream. Of these eight specimens the sex

* Jardine's "Naturalist's Library, Mammalia," vol. vi. Cetacea. Edinburgh, 1837.

† March 10, 1770.

of three was either not recognised or has not been stated. Five, however, are known to have been males—a circumstance of considerable interest, as it serves to corroborate the statement made by Mr Thomas Beale, in his work on the natural history of the sperm whale, that "the large and fully-grown males always go singly in search of food."

5. On the Efficient Powers of Parturition. By Dr J. Matthews Duncan.

There can be no doubt that, among the numerous matters at present occupying the attention of obstetricians, none is more important than the subject of this paper. So evident is the correctness of this statement that one cannot but wonder why attempts to arrive at the truth have been, so far as we know, delayed till the present day. It is long since excellent researches of an analogous kind in regard to the force of the circulation of the blood, the power of the ventricles of the heart, were published; yet such researches do not seem naturally so attractive, nor do they give promise of so valuable practical results as those into the power of labour.

It is well known that the first and, I believe, the best results in this inquiry have been obtained by careful deduction from experiments on the tensile strength of the amniotic membrane. The researches referred to were made quite independently, and published soon after one another by Poppel, of Munich, and by Tait and myself conjointly. Studying this subject, I thought of some other modes of reaching conclusions, such as by observations on the caput succedaneum. Means might be taken to find the force required to raise a caput succedaneum, and the variations of force required to raise this swelling in different degrees of thickness. Such an investigation would, no doubt, lead to similar valuable results, but the plan has never been employed. Again, observations might be made to ascertain the force required to rupture the fourchette or the perineum, and thus a fact might be got which would be of service in this inquiry. It is well known to accoucheurs how these parts sometimes offer a successful resistance to all the powers of labour. This resistance, if its force be ascer-

tained, is of course a measure of the power employed; at least, it would afford a valuable result as to the limits of the power. Like statements might be made regarding the laceration of the margin of the cervix uteri, as a test of the power exerted at the completion of the first stage of labour. Many methods were available, but none were till very recently worked out.

It is probable that many intelligent and thoughtful accoucheurs had some rough ideas as to the amount of power exerted in parturition. They could not fail, in attending on ordinary labours, to observe the strength of hand and arm required to keep back the head too rapidly advancing over a delicate perineum. This power is, under certain conditions, a measure of the force of the labour, but I am not aware that any one has hitherto made the simple and proper dynamometrical experiments to decide the amount of force so exerted by the accoucheur. The problem may be more exactly stated as follows:—If in an unobstructed and powerful labour, the accoucheur, by the directly opposing pressure of his hand on the fœtal head, arrests its progress for one or several pains, he has in the pressure of his hand a force which, added to the small amount required to effect parturition, exceeds all the combined powers of labour in this case. He may then estimate by dynamometrical experiment what was the force he used, or what force he is capable of applying in the way in which he actually applied it to arrest the progress of labour. This experiment may be varied in different ways, of which I may mention one. Let us suppose a case of rigid vulva, the perineal resistance being overcome, and the head retroceding during the interval between powerful bearing down pains. Now, it is well known that in such a case a little manual pressure from above may be enough to push the head down again on the perineum, or to resist retrocession, or that the first and painless part of the next pain will make the head that has retroceded, again bulge out the perineum, before it is forced by the powerful acme of the pain against the resisting vulva. If, then, the practitioner opposes the advance of the head even so far as to bulge out the perineum, he must have a nearly exact measure of the force which the labour could bring to bear against the vulvar obstacle.

In such experiments or practice, what force does the accoucheur

exert? I have a hand well accustomed to such work, and I find, by actual trial with an accurate dynamometer, 50 lbs. to be about the highest power I can use, situated as I am at the bedside in attendance on a case. I have ample reason, then, in such experience to believe that very few of the most powerful labours exert a force of 50 lbs.; that an ordinary strong labour is easily arrested by a much smaller force than 50 lbs.; that the great majority of labours is accomplished by repeated efforts whose highest power never exceeds 25 lbs. I may add that, in the great mass of short forceps deliveries, the force required from the accoucheur, even when he delivers the head, unaided by the natural efforts, seldom reaches 50 lbs. These statements are, to a great extent, arbitrary or dependent on my skill as an observer, yet I feel very confident of their accuracy.

Again, the intelligent practitioner who has observed a case of difficult labour finished either by the long forceps or by podalic extraction, could not but form some rough idea of the force he used, and compare it with the force which the labour exerted in its nugatory struggles. The force which the accoucheur thus exerted would not be certainly the equivalent of what the labour must have put forth in order to produce a spontaneous termination. It would, no doubt, in most cases surpass the force which the mother must have exerted to produce the spontaneous birth. But it would be, nevertheless, a valuable measurement indicating a force which in such a case the labour failed to produce. Joulin and I have made dynamometrical experiments to make use of such measurements in estimating the highest power of labour.

Another method of advancing our knowledge of this subject has been followed by the Rev. Professor Haughton. This gentleman does not, as his predecessors, examine the effects produced by the powers of labour, and thus get results having a very distinct positive value. He follows a plan which may be justifiable, yet which is difficult and dangerous. He takes an almost opposite method to that used by me. He measures the bulk and the extent of the involuntary and voluntary muscles employed in the function, and from these data he arrives at conclusions which he in one particular corroborates by a simple experiment. The results arrived at are statements of the powers of the parts, which are true if his methods

are true. Even if his methods are correct, the results are not actual values, but possible values, or statements of what may be, not of what has been.

These results are very different from those of Poppel, Tait, and myself, and it is one of the objects of this paper to inquire into their value. In doing this, I shall not discuss the method, but merely examine the results, by the aid of any obstetrical light which I can throw upon them.

Before proceeding to this inquiry, it is to be remarked that Haughton arrives by his method at new results which the methods of previous observers did not afford the means of reaching. There are, as is universally known, two great forces employed in labour—the uterine contractions and the involuntary and voluntary bearing down. The former of these forces is peculiar to the parturient female. The latter, as Haughton truly observes, is not peculiar to parturition, but is "available to expel feces, urine, or a fœtus." Haughton's plan is, to examine the uterus, measure it, and through this, arrive at a conclusion as to its power; then to examine the muscles which co-operate to produce bearing down, measure them, and through this arrive at a conclusion as to their power. The addition of the two results will, of course, give the power of labour. As I have already said, this is a dangerous and difficult plan to follow, and this is because there is room for error at every step.

The conclusions which Poppel and Tait and myself enunciated regarding the power of natural parturition stand on a completely different and, it appears to me, far more secure footing. There can, indeed, be scarcely any important difficulty raised regarding them. The strength of the fœtal membranes is ascertained by experiment. Certain facts are well known regarding the rupture of the membranes generally, and regarding their rupture in the labours in which the membranes experimented on were produced. These two sets of data, when put together, lead by a process of reasoning, which it would be tedious here to recapitulate, to conclusions regarding the lower limit of the power of natural labour, and regarding the power of labour generally, which cannot, so far as I see, be cavilled at. It is evident that this method tests only the whole or the combined powers of labour. It can afford no hint

as to the comparative value of the two forces which combine to produce the power which is to be measured.

The results given in Professor Haughton's paper which appear to me to be both new and important are three. I shall first state them, and then proceed to their examination one by one :—

1. The first conclusion is, that " the uterine muscles are capable of rupturing the membranes in every case, and possess in general nearly three times the amount of force requisite for this purpose." . . . . " It would be a waste of power (adds Haughton) to endow the uterus with more force than I have shown it to possess, for it is not necessary that the uterus should complete the second stage of labour, as the abdominal muscles are available for this purpose ; so that by using them, and not giving the uterus more force than is absolutely necessary for the first stage of labour, an admirable economy of muscular power is effected." . . . " The extreme force of uterine contraction produces a pressure of 3·402 lbs. per square inch, which is equivalent to a pressure of 54·106 lbs. acting upon a circle of four and a-half inches in diameter, which is assumed as the average area of the pelvic canal."

2. The second of Professor Haughton's new and important conclusions is, that the action of the voluntary abdominal muscles " constitutes the chief part of the force employed in difficult labours." . . . " The amount of available additional force given out by the abdominal muscles admits of calculation, and will be found much greater than the force produced by the involuntary contractions of the womb itself."

3. The third conclusion is, " that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner." . . . " Adding together the combined forces of the voluntary and involuntary muscles, we find—

Involuntary muscles . .	=	54·10 lbs.
Voluntary muscles . .	=	523·65 lbs.
Total . .		577·75 lbs. av."

I. The first of Professor Haughton's conclusions on which I comment is, to the effect that the unaided uterine muscle can

exert a force in labour of 54 lbs., that this force is employed in dilating the cervix and rupturing the membranes, and that it can or does effect little more.

Now, it appears to me that Haughton limits far too much the use of the power of the uterus. I have no doubt that the uterine efforts not only dilate the cervix and rupture the membranes in most cases, but also do, in most cases, perform the chief part of the work required to bring forth the child. Although I do not coincide with Haughton in his reflections on the economy of muscular power, I shall not discuss the point therein raised. Yet I cannot avoid saying that, in the present instance, his own statements invalidate his reflections, for he asserts that the uterine muscle has three times the amount of muscular power required to do the work demanded of it. In endowing the uterus with this great power, Haughton, in my opinion, furnishes conclusive evidence against his own view as to the use of the contractions of the uterus. For I am sure that the great mass of births, even in difficult labours, including only the most difficult, is effected by a force less than what Haughton ascribes to the uterine muscle alone. I am satisfied that the whole combined powers of labour seldom reach above 50 lbs., while Haughton gives the uterus alone a power of 54.

I do not say Haughton is wrong in supposing that the uterus can exert a force of 54 lbs. On the contrary, I have no reason to doubt it. But I am sure that while easy labours require for their whole work a force scarcely exceeding the weight of the child, only a few difficult labours require for their whole work a force exceeding 50 lbs.

Every accoucheur knows to some degree of exactness the force which is required to restrain the forward movement of the child when there is no special resistance to its advance. This power I have measured approximatively by dynamometrical experiments, and I find it to be at the most 50 lbs.,—a power less than what is ascribed by Haughton to the unaided uterus. In other words, the uterus and voluntary muscles combined, stimulated to violent effort by insuperable temporary resistance, exert a force greater than is required to complete the labour; yet this force is generally much less than 50 lbs., and possibly never exceeds it.

It is well-known to accoucheurs that the great resistance to the progress of the child in the second stage of labour is what is called in obstetrics the perineum. The power of this part I do not know, and guessing is a bad proceeding in a scientific paper. Yet I may venture to say that no perineum would long resist a force of 50 lbs. repeatedly applied, a force less than Haughton ascribes to the uterine muscle.

II. Haughton's second conclusion is that the chief force in parturition is furnished by the voluntary muscles. The available power of these is (he says) 523 lbs., while that of the uterus is 54. The whole amount of expulsive force of the voluntary muscles is, he says, not usually employed to assist the uterus in completing the second stage of labour; but this does not contradict the conclusion we have ascribed to him. The conclusion is indeed, for Professor Haughton, inevitable, for every accoucheur knows that the bearing down efforts, whatever may be their actual measured power, are very strong, perhaps as strong as possible, quite frequently in ordinary labours. Besides, Haughton himself expounds his meaning in the following words:—"It is plainly necessary that the first stage in the expulsion of the fœtus should not be intrusted to a voluntary muscle, and hence an involuntary muscle is gradually provided, which takes the initiative and commences the process of parturition, the completion of which is then accomplished by the aid of voluntary muscles, to the employment of which, at this stage, no moral objection can be raised. It is also necessary (if the Contriver be allwise, or if the principle of least action in nature be true), that the involuntary muscle so produced, should not possess more or less force than is requisite for its purpose. The uterine muscle does not grow to meet a growing resistance (as happens frequently in other cases), and its precise degree of strength cannot be produced by a tentative process; for in healthy gestation the uterine muscle never tries its force against the membranes it is called upon to rupture until the actual period of parturition has arrived."

The view expounded in these words has great authority on its side beside that of the quoted writer, for the point therein raised as to the relative powers and uses of the uterine and auxiliary

forces of parturition is one that has been much discussed and for a long time. The great Haller, indeed, held opinions which are in accordance with Haughton's view. This renowned physiologist discarded the opinion common in his day, and now almost universally entertained, that the uterus is the main source of the power exerted in every stage of parturition.

Haughton gives us no reason for discrediting the general opinion of obstetricians, relying apparently on his conclusions alone regarding the comparative power of the two forces, that of the uterine muscle and that of the assistant voluntary muscles. No doubt he makes some observations intended to be corroborative as to the economy of force and other so-called laws of nature; but such reflections cannot be regarded otherwise than as premature by those who, like myself, do not adopt this writer's conclusions upon whose verity their justice depends.

In the course of his concise view of this question in his work on Physiology, Haller twice takes care to express his doubts as to the truth of his own opinions; and he ends by appealing to anatomists for light upon the subject. This appeal is, at least, ingenuous, for his argument against the ordinary opinion rests greatly upon the uterine fibres, their direction, and the direction of the force evolved by them; and, as Haller's notions on this anatomical point were very imperfect, and his mechanical ideas equally so, we need attach no weight to this part of his argument. Besides this, however, he has really nothing deserving the name of good evidence on his side. He thinks the effects produced by expulsive pains greater than the power of the uterus; but this is evidently mere begging the question. So also is his dependence, for aid in his judgment, on a picture of the great struggles of the voluntary muscles.

Authors generally do, as I have said, entertain an opinion opposed to that of Haller and Haughton. They are too numerous to name, and no one merits special mention; for, so far as I know, no one has distinguished himself by the novelty or elaborateness of his arguments in support of the ordinary view that the uterus is the chief agent in the whole process of parturition, and that the voluntary muscles, whether stimulated by volition or by reflex excitement, are, in a secondary position, aiding the uterus indeed, but not supplying the chief force. There is no positive value in an

argument of appeal to authority, yet it is evident that the amount of authority against him made Haller hesitate to enunciate his own views; and, when we consider the number, the intelligence, and the acute attention of the obstetricians who form a majority, scarcely differing from the whole body, in favour of our view, we cannot but be weightily impressed in its favour.

I must admit that some of the arguments made by obstetric authors to do regular service in defence of their view are very weak or quite vain. I may cite examples. Cases of parturition completed when the uterus is prolapsed, and is said to derive no assistance from bearing down efforts, are cited. But such cases prove almost nothing, even supposing they are correctly described; for there is in such cases absence of the ordinary difficulties of labour which consist in the propulsion of the child through the pelvis. Cases of expulsion of the child after death of the mother are quoted. But so far as I have perused them, they are given with a deficiency of circumstantial data such as to invalidate them altogether. Indeed, it is, in some of them, not even shown that the uterus acted at all; while in all there is the assumption that the difficulty of birth after death is as great as before it. The like objections may be made to examples of labour in asphyxia, narcotism, and syncope. It has been asserted also that narcotism by chloroform affords evidence that the uterus is the chief agent in parturition. But I must assert the incorrectness of this argument, and I cannot understand why Haughton should call attention to the influence of this agent, for any argument from it is valid, so far as it goes, only against his own views. I have, in a large experience, never seen chloroform inhalation destroy the action of the voluntary muscles. I believe it generally weakens their action, and it is well known that, at the worst, it only weakens the powers of labour. It is not known whether it weakens the uterine action or the action of the voluntary muscles in the greatest degree. If it does, as is alleged, when given profusely, destroy the action of the voluntary muscles, it certainly seldom completely arrests the progress of labour. Lastly, cases of labour in paraplegic women are cited in favour of the ordinary opinion. But I fear they do not even appear to favour it; and, with a view to the present question, they cannot be held as settling anything, seeing we do not know

what influence paraplegia may exert on the uterus itself. Besides, the cases are insufficient in every way.

The arguments on which I place chief reliance are the following:—

1. The great power of the uterus felt by the hand of the accoucheur, as in the operation of turning, long after the rupture of the membranes.

2. The great and sufficient power of the uterus observed in cases where the action of the voluntary muscles is weak or restrained.

3. The regulating influence of purely uterine pains in the progress of the second stage of labour.

4. The supremely important demand for and presence of powerful uterine action after the expulsion of the child.

5. The arrest of the progress of labour by inertia of the uterus. This argument appears to me unanswerable, for the condition often occurs when there is certainly only the slightest possible resistance to the progress of the child, when the mother ardently desires the completion of labour, and bears down violently with this object in view.

6. In cases of uterine inertia, such as are above described, the practitioner may find, by pulling with the forceps from below or pushing with the hands from above, in the absence of all parturient effort, whether of the uterus or of the voluntary muscles, that a very small force, say not exceeding the weight of the child, is sufficient to finish a labour upon whose progress violent bearing down efforts have had no effect.

7. The circumstance that, were the voluntary muscles the chief agents, expulsion of the child would be in great part a voluntary act, which it certainly is not.

8. The asserted completeness of the function of parturition in animals in which the assistant bearing down efforts are annihilated by opening the abdomen; the process being effected by their uterine and vaginal muscles, which are weak when compared with that of women.

Baudelocque and Velpeau* relate cases which appear to show that woman has very rarely voluntary power over the progress of parturition for a time Such cases offer no difficulty when regarded

* Traité complét de l'art des Accouch. Ed. Bruxelles, p. 227.

with a view to the present question. They are explicable in more ways than one, and an illustrative statement is, for my present purpose, quite sufficient. Every experienced accoucheur has seen cases where voluntary increase of bearing down has sufficed to expedite labours, which, if the women had been left in a sleepy, lethargic condition, might have been protracted for an indefinite length of time.

There can be no doubt that the uterus is a very powerful agent in expelling the fœtus from its cavity into the world—that it is not the sole agent, and that it is assisted by the action of the voluntary muscles. Though I have not proved absolutely that the uterus is the chief agent in the performance of this function, yet I have no doubt that it is so; and I think that the arguments I have adduced give this belief of the profession the highest degree of probability. This belief does not imply that the aid afforded by the voluntary muscles is inconsiderable or unimportant. It only renders it quite incredible that while the power of the uterus is 54 lbs. that of the voluntary muscles can be 523.

III. Haughton's conclusion, on which I wish last of all to comment, is, "that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner."

In my work entitled "Researches in Obstetrics," to which Professor Haughton refers, I have discussed carefully, but briefly, this point, and announce the conclusion that the comparatively small figure of 80 lbs. gives the highest power of labour; and I quote Joulin, who estimates it at somewhat above 100 lbs. I do not deny that in exceptional circumstances a few pounds above 80 may be reached, but I feel pretty sure that seldom in the history of woman has the figure 80 been attained, whether on an emergency or not. This conclusion is arrived at by experiment and observation—experiments on the force required to pull a child through a contracted brim of pelvis, observations of the force used to complete a difficult labour, which nature, in its most violent throes, has failed to accomplish.

Every accoucheur will, I suppose, readily admit that, in a case

of delivery by podalic extraction, the surgeon can exert a great deal more force to bring the child into the world than the most energetic labour can. Now, in these circumstances the surgeon can use no force nearly reaching to a quarter of a ton. A very much smaller power would rend the luckless body of the child in pieces.

Such a power as a quarter of a ton does, in my opinion, represent a force to which the maternal machinery could not be subjected without instantaneous and utter destruction. To speak of a rigid perineum resisting such a power, or the fourth part of it, would be ridiculous. The possession and use even of a considerable portion of such a power would render the forceps and the cephalotribe weak and useless instruments. The mother could bray the child as in a mortar, and squeeze it through a pelvis which would, under other circumstances, necessitate Cæsarean section. Such a power would, if appropriately applied, not only expel the child, but also lift up the mother, the accoucheur, and the monthly nurse all at once. It would be dangerous not only to the mother and the child; it would imperil also the accoucheur. It has been calculated for me, that if this force were applied just as the chief resistance to delivery was overcome, the child would be shot out of the vagina at the rate of thirty-six feet per second!* The blow would be equal to the shock produced by the fall of the child from a height of twenty-one feet.

In an early part of this paper I have said that the method of inquiring into the subject which Haughton adopts is both difficult and dangerous, and I think I have said enough to show that danger has not been avoided. There must be error in Professor Haughton's calculation of the power produced by the action of the voluntary muscles, or there must be error in judging of the application of this power to the accomplishment of the function, or there must be error in both. I shall not attempt to show where the error lies, but its occurrence does not astonish me; for any one

* In making this calculation the child is taken as 7 lbs., the pressure as 580 lbs., and it is supposed to be exerted through a space of three inches—measurements which are fair statements of the case. It is farther supposed that the friction is negligible when compared with the forward pressure. This is certainly the case if the forward pressure be nearly as much as is stated by Professor Haughton as possible.

who has studied the difficult subject of the retentive power of the abdomen will recognise the difficulty of reaching conclusions as to the power of labour by Haughton's method. It is highly probable that the power of the voluntary muscle is dissipated, perhaps in compressing intestinal gases, perhaps in consequence of being misdirected.

Whatever may be the real source of error as to this matter, it is highly desirable to find it out, in order that we may, by more accurate proceedings, arrive at the true results which Haughton hoped to reach.

The following Gentlemen were admitted Fellows of the Society :—

Rev. WILLIAM SCOTT MONCRIEFF, of Fossaway, M.A. (Camb.)
Professor A. R. SIMPSON.
Dr R. J. BLAIR CUNYNGHAME.
Dr COSMO GORDON LOGIE, Surgeon-Major, Royal Horse Guards.

Monday, 20th February 1871.

W. F. SKENE, LL.D., Vice-President, in the Chair.

The following Communications were read :—

1. On the Pentatonic and other Scales employed in Scottish Music. By the Hon. Lord Neaves.

Lord Neaves adverted to the peculiarity which had been observed in many Scotch airs, that they are composed on a pentatonic scale, and do not make use of the fourth or seventh of the gamut. It has been said that these airs can be played on the black notes of the pianoforte, which means that they can be played on the key of F♯ major, of which the fourth and seventh are represented by white notes, but are not needed. He also observed that this class of airs could be played on the white notes of the piano, both in the key of F and in that of G. They could be played on F, because, as they do not use the fourth, they do not need B♭; and they could be played on G, because, as they do not use the seventh, they do not need F♯. They could also, of course, be played on the key of C.

Many minor airs can be played on the pentatonic scale of the relative major; that is, airs on D♯ minor can be played on the black notes, and airs in A minor can be played on the white notes on the pentatonic of C; airs in D minor on the pentatonic of F; and airs in E minor on the pentatonic of G.

Specimens of major pentatonic airs are these—"Roy's Wife," "Auld Langsyne," "Ye Banks and Braes," "The Gypsies came," "Whistle o'er the lave o't."

Specimens of minor pentatonic airs—"The Mucking o' Geordie's byre," "My tocher's the jewel," "Auld Robin Gray" (old set), "Wandering Willie," "Ca' the yowes to the knowes."

Some minor airs are composed on the pentatonic of the tone below.

Specimens—"Adieu, Dundee" (in Skene MS.), "Blythe, Blythe."

In several old pentatonic airs grace notes or transitional notes have been added in modern singing or playing, but the original pentatonic character can still be traced.

Another large class of Scotch airs are composed on the full diatonic scale, and can be played entirely on the *white* notes without any apparent modulation.

When these airs are on the key of C major, there is nothing very peculiar in them, and there are many of this class. But when they are composed on other keys, certain peculiarities appear.

Several Scotch airs are composed in the key of G, but played on the full diatonic scale of C, so as frequently to introduce F natural, sometimes with a pathetic, sometimes with a comic effect. The old set of the "Flowers of the Forest" (Skene MS.) is an example of the one, and the tune of "Pease Strae" of the other.

Other specimens are—"Bessie Bell," "Tullochgorum," "Lochaber no more."

Minors in the diatonic scale are often singular, as, for instance, the air of "My boy, Tammie," played on the white notes. It runs into three keys—D minor, C major, and F major.

The pentatonic scale is not peculiar to Scotch music, but it may partly be accounted for by the fact that rude wind instruments are apt to be defective in the fourth and fifth. The simple diatonic scale, without other semitones, may in like manner have been used

from the adoption of early harps or other stringed instruments of a limited construction.

Scotch airs were often imitated by introducing a particular accentuation, called the Scottish "snap," as in the Vauxhall air, "'Twas within a mile of Edinburgh Town."

He expressed an opinion that many airs were common to Scotland and the North of England, and he denied that Scotch airs were always sombre, as had sometimes been alleged.

Airs illustrating the views above stated were played by Mr Bridgman in a manner of which it may be allowable to say that it gave great satisfaction to the audience.

2. On the Motion of Free Solids through a Liquid. By Sir William Thomson.

This paper commences with the following extract from the author's private journal, of date January 6, 1858:—

" Let $\mathfrak{X}, \mathfrak{Y}, \mathfrak{Z}, \mathfrak{L}, \mathfrak{M}, \mathfrak{N}$ be rectangular components of an impul-
" sive force and an impulsive couple applied to a solid of invariable
" shape, with or without inertia of its own, in a perfect liquid,
" and let $u, v, w, \varpi, \rho, \sigma$, be the components of linear and angular
" velocity generated. Then, if the vis viva* (twice the mechanical
" value) of the whole motion be, as it cannot but be, given by the
" expression

" $Q=[u,u]u^2+[v,v]v^2+\ldots.+2[v,u]vu+2[w,u]wu+2[\varpi,u]\varpi u+\ldots.$

" where $=[u,u]$, $[v,v]$, &c., denote 21 constant co-efficients determin-
" able by transcendental analysis from the form of the surface of
" the solid, probably involving only elliptic transcendentals when
" the surface is ellipsoidal: involving, of course, the moments of
" inertia of the solid itself: we must have

$$[u,u]u+[v,u]v+[w,u]w+[\varpi,u]\varpi+[\rho,u]\rho+[\sigma,u]\sigma=\mathfrak{X}, \text{ \&c.}$$
$$[u,\varpi]u+[v,\varpi]v+[w,\varpi]w+[\varpi,\varpi]\varpi+[\rho,\varpi]\rho+[\sigma,\varpi]\sigma=\mathfrak{L}, \text{ \&c.}$$

" If now a continuous force X,Y,Z, and a continuous couple
" L,M,N, referred to axes fixed in the body, is applied, and if
" $\mathfrak{X}$ . . . . . &c., denote the impulsive force and couple capable
" of generating from rest the motion $u, v, w, \varpi, \rho, \sigma$, which exists

* Henceforth T, instead of $\frac{1}{2}$Q, is used to denote the "mechanical value," or, as it is now called, the "kinetic energy" of the motion.

"in reality at any time t; or merely mathematically, if $\mathfrak{X}$, &c., "denote for brevity the preceding linear functions of the com- "ponents of motion, the equations of motion are as follow:—

$$\left.\begin{array}{l} \dfrac{d\mathfrak{X}}{dt} - \mathfrak{Y}\sigma + \mathfrak{Z}\rho = \mathrm{X}\,, \dfrac{d\mathfrak{Y}}{dt} = \text{\&c., \&c.} \\ \dfrac{d\mathfrak{L}}{dt} - \mathfrak{Y}w + \mathfrak{Z}v - \mathfrak{M}\sigma + \mathfrak{N}\rho = \mathrm{L} \\ \dfrac{d\mathfrak{M}}{dt} - \mathfrak{Z}u + \mathfrak{X}w - \mathfrak{N}\varpi + \mathfrak{L}\sigma = \mathrm{M} \\ \dfrac{d\mathfrak{N}}{dt} - \mathfrak{X}v + \mathfrak{Y}u - \mathfrak{L}\rho + \mathfrak{M}\varpi = \mathrm{N} \end{array}\right\} \quad . \quad . \quad . \quad (1)$$

"Three first integrals, when

$$\mathrm{X} = 0,\ \mathrm{Y} = 0,\ \mathrm{Z} = 0,\ \mathrm{L} = 0,\ \mathrm{M} = 0,\ \mathrm{N} = 0,$$

"must of course be, and obviously are,

$$(2)\ \mathfrak{X}^2 + \mathfrak{Y}^2 + \mathfrak{Z}^2 = \text{const.}$$

"resultant momentum constant;

$$(3)\ \mathfrak{L}\mathfrak{X} + \mathfrak{M}\mathfrak{Y} + \mathfrak{N}\mathfrak{Z} = \text{const.}$$

"resultant of moment of momentum constant; and

$$(4)\ u\mathfrak{X} + v\mathfrak{Y} + w\mathfrak{Z} + \varpi\mathfrak{L} + \rho\mathfrak{M} + \sigma\mathfrak{N} = \mathrm{Q}\ .\text{"}$$

These equations were communicated in a letter to Professor Stokes, of date (probably January) 1858, and they were referred to by Professor Rankine, in his first paper on Stream Lines, communicated to the Royal Society of London,* July 1863.

They are now communicated to the Royal Society of Edinburgh, and the following proof is added:—

Let P be any point fixed relatively to the body, and at time t, let its co-ordinates relatively to axes OX,OY,OZ fixed in space, be

* These equations will be very conveniently called the Eulerian equations of the motion. They correspond precisely to Euler's equations for the rotation of a rigid body, and include them as a particular case. As Euler seems to have been the first to give equations of motion in terms of co-ordinate components of velocity and force referred to lines fixed relatively to the moving body, it will be not only convenient, but just, to designate as "Eulerian equations" any equations of motion in which the lines of reference, whether for position, or velocity, or moment of momentum, or force, or couple, move with the body, or the bodies whose motion is the subject.

x, y, z. Let PA, PB, PC be three rectangular axes fixed relatively to the body, and (A,X), (A,Y), . . . the cosines of the nine inclinations of these axes to the fixed axes OX, OY, OZ.

Let the components of the "impulse"* or generalized momentum, parallel to the fixed axes be ξ, η, ζ, and its moments round the same axes λ, μ, ν, so that if X, Y, Z be components of force acting on the solid, in line through P, and L, M, N components of couple, we have

$$\left.\begin{aligned}&\frac{d\xi}{dt} = \mathrm{X}, \quad \frac{d\eta}{dt} = \mathrm{Y}, \quad \frac{d\zeta}{dt} = \mathrm{Z}\\ &\frac{d\lambda}{dt} = \mathrm{L} + \mathrm{Z}y - \mathrm{Y}z, \quad \frac{d\mu}{dt} = \mathrm{M} + \mathrm{X}z - \mathrm{Z}x, \quad \frac{d\nu}{dt} = \mathrm{N} + \mathrm{Y}x - \mathrm{X}y\end{aligned}\right\} \quad (6).$$

Let $\mathfrak{X}$, $\mathfrak{Y}$, $\mathfrak{Z}$ and $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$ be the components and moments of the impulse relatively to the axes PA, PB, PC moving with the body. We have

$$\left.\begin{aligned}&\xi = \mathfrak{X}(\mathrm{A}, \mathrm{X}) + \mathfrak{Y}(\mathrm{B}, \mathrm{X}) + \mathfrak{Z}(\mathrm{C}, \mathrm{X})\\ &\ldots\ldots\ldots\ldots\\ &\ldots\ldots\ldots\ldots\\ &\lambda = \mathfrak{L}(\mathrm{A}, \mathrm{X}) + \mathfrak{M}(\mathrm{B}, \mathrm{X}) + \mathfrak{N}(\mathrm{C}, \mathrm{X}) + \mathfrak{Z}y - \mathfrak{Y}z\\ &\ldots\ldots\ldots\ldots\\ &\ldots\ldots\ldots\ldots\end{aligned}\right\} \quad (7).$$

Now let the fixed axes OX, OY, OZ be chosen coincident with the position at time t of the moving axes, PA, PB, PC, we shall consequently have

$$\left.\begin{aligned}&x = 0, \; y = 0, \; z = 0,\\ &\frac{dx}{dt} = u, \; \frac{dy}{dt} = v, \; \frac{dz}{dt} = w,\end{aligned}\right\} \quad (8).$$

$$\left.\begin{aligned}&(\mathrm{A}, \mathrm{X}) = (\mathrm{B}, \mathrm{Y}) = (\mathrm{C}, \mathrm{Z}) = 1\\ &(\mathrm{A}, \mathrm{Y}) = (\mathrm{A}, \mathrm{Z}) = (\mathrm{B}, \mathrm{X}) = (\mathrm{B}, \mathrm{Z}) = (\mathrm{C}, \mathrm{X}) = (\mathrm{C}, \mathrm{Y}) = 0\\ &\frac{d(\mathrm{A}, \mathrm{Y})}{dt} = \sigma, \quad \frac{d(\mathrm{B}, \mathrm{X})}{dt} = -\sigma, \quad \frac{d(\mathrm{C}, \mathrm{X})}{dt} = \rho\\ &\frac{d(\mathrm{A}, \mathrm{Z})}{dt} = -\rho, \quad \frac{d(\mathrm{B}, \mathrm{Z})}{dt} = \varpi, \quad \frac{d(\mathrm{C}, \mathrm{Y})}{dt} = -\varpi,\end{aligned}\right\} \quad (9).$$

Using (7), (8), and (9) in (6) we find (1).

* See "Vortex Motion," § 6, Trans. Roy. Soc. Edin. (1868).

One chief object of this investigation was to illustrate dynamical effects of heliçoidal property (that is right or left-handed asymmetry). The case of complete isotropy, with heliçoidal quality, is that in which the coefficients in the quadratic expression for T fulfil the following conditions.

$$\left.\begin{array}{l} [u,u] = [v,v] = [w,w] \quad \text{(let } m \text{ be their common value)} \\ [\varpi,\varpi] = [\rho,\rho] = [\sigma,\sigma] \quad\quad ,, \; n \quad ,, \quad ,, \quad ,, \\ [u,\varpi] = [v,\rho] = [w,\sigma] \quad\quad ,, \; h \quad ,, \quad ,, \quad ,, \\ [v,w] = [w,u] = [u,v] = 0 ; \;\; [\rho,\sigma] = [\sigma,\varpi] = [\varpi,\rho] = 0 \\ \text{and } [u,\rho] = [u,\sigma] = [v,\sigma] = [v,\varpi] = [w,\varpi] = [w,\rho] = 0 \end{array}\right\} \quad (10).$$

so that the formula for T is

$$T = \tfrac{1}{2}\{m(u^2+v^2+w^2)+n(\varpi^2+\rho^2+\sigma^2)+2h(u\varpi+v\rho+w\sigma)\} \quad . \quad (11).$$

For this case therefore the Eulerian equations (1) become

$$\left.\begin{array}{l} \dfrac{d(mu+h\varpi)}{dt} - m(v\sigma - w\rho) = X, \text{ \&c.} \\ \text{and } \dfrac{d(n\varpi+hu)}{dt} = L, \text{ \&c.} \end{array}\right\} (11).$$

[Memorandum:—Lines of reference fixed relatively to the body].

But inasmuch as (11) remains unchanged when the lines of reference are altered to any other three lines at right angles to one another through P, it is easily shown directly from (6) and (9), that; if, altering the notation, we take u, v, w to denote the components of the velocity of P parallel to three fixed rectangular lines, and ϖ, ρ, σ the components of the body's angular velocity round these lines, we have

$$\left.\begin{array}{l} \dfrac{d(mu+h\varpi)}{dt} = X, \text{ \&c.} \\ \text{and } \dfrac{d(n\varpi+hu)}{dt} - h(\sigma v - \rho w) = L, \text{ \&c.} \end{array}\right\} (12).$$

[Memorandum:—Lines of reference fixed in space], which are more convenient than the Eulerian equations.

The integration of these equations, when neither force nor couple acts on the body ($X = 0$, &c.; $L = 0$, &c.), presents no difficulty, but its result is readily seen from § 21 ("Vortex Motion") to be that, when the impulse is both translatory and rotational, the point P, round which the body is isotropic, moves

uniformly in a circle or spiral so as to keep at a constant distance from the "axis of the impulse," and that the components of angular velocity round the three fixed rectangular axes are constant.

An isotropic heliçoid may be made by attaching projecting vanes to the surface of a globe, in proper positions; for instance, cutting at 45° each at the middles of the twelve quadrants of any three great circles, dividing the globe into eight quadrantal triangles. By making the globe and the vanes of light paper, a body is obtained rigid enough and light enough to illustrate by its motions through air the motions of an isotropic heliçoid through an incompressible liquid. But curious phenomena, not deducible from the present investigation, will no doubt, on account of viscosity, be observed.

Part II.

Still considering only one movable rigid body, infinitely remote from disturbance of other rigid bodies, fixed or movable; let there be an aperture or apertures through it, and let there be irrotational circulation or circulations (§ 60 " Vortex Motion ") through them. Let ξ, η, ζ, be the components of the " impulse " at time t, parallel to three fixed axes, and λ, μ, ν its moments round these axes, as above, with all notation the same, we still have (26 " Vortex Motion ")

$$\left.\begin{aligned} \frac{d\xi}{dt} &= \mathrm{X}, \text{ \&c.} \\ \frac{d\lambda}{dt} &= \mathrm{L} + \mathrm{Z}y - \mathrm{Y}z, \text{ \&c.} \end{aligned}\right\} \quad . \ . \ . \ (6) \text{ (repeated).}$$

But, instead of for T a quadratic function of the components of velocity as before, we now have

$$\mathrm{T} = \mathrm{E} + \tfrac{1}{2}\{[u, u]\,u^2 + \ . \ . \ . + 2[u, v]uv + \ . \ . \ .\} \ . \ . \ . \ (13).$$

where E is the kinetic energy of the fluid motion when the solid is at rest, and $\frac{1}{2}\{[u, u]u^2 + \ . \ . \ .\}$ is the same quadratic as before. The coefficients $[u, u]$, $[u, v]$, &c., are determinable by a transcendental analysis, of which the character is not at all influenced by the circumstance of there being apertures in the solid. And instead of $\xi = \dfrac{d\mathrm{T}}{du}$, &c., as above, we now have

$$\left.\begin{array}{l} \xi = \dfrac{d\mathrm{T}}{du} + \mathrm{I}l,\ \eta = \dfrac{d\mathrm{T}}{dv} + \mathrm{I}m,\ \zeta = \dfrac{d\mathrm{T}}{dw} + \mathrm{I}n \\ \lambda = \dfrac{d\mathrm{T}}{d\varpi} + \mathrm{I}(ny - mz) + \mathrm{G}l,\ \mu = \&\mathrm{c.},\quad \nu = \&\mathrm{c.} \end{array}\right\} \ldots (14),$$

where I denotes the resultant "impulse" of the cyclic motion when the solid is at rest; l, m, n its direction cosines; G its "rotational moment," (§ 6, "Vortex Motion"); and x, y, z the co-ordinates of any point in its "resultant axis." These (14) with (13) used in (6) give the equations of the solid's motion, referred to fixed rectangular axes. They have the inconvenience of the coefficients $[u, u]$, $[u, v]$, &c., being functions of the angular co-ordinates of the solid. The Eulerian equations (free from this inconvenience) are readily found on precisely the same plan as that adopted above for the old case of no cyclic motion in the fluid.

The formulæ for the case in which the ring is circular, has no rotation round its axis, and is not acted on by applied forces, though of course easily deduced from the general equations (14), 13), (6), are more readily got by direct application of first principles. Let P be such a point in the axis of the ring, and $\mathfrak{C}$, A, B, such constants that $\frac{1}{2}(\mathfrak{C}\omega^2 + \mathrm{A}u^2 + \mathrm{B}v^2)$ is the kinetic energy due to rotational velocity ω round D, any diameter through P, and translational velocities u along the axis and v perpendicular to it. The impulse of this motion, together with the supposed cyclic motion, is therefore compounded of

$$\text{momentum in lines through P}\left\{\begin{array}{l}\mathrm{A}u + \mathrm{I} \text{ along the axis} \\ \mathrm{B}v \text{ perpendicular to } ,, \ ,, ,\end{array}\right.$$

and moment of momentum $\mathfrak{C}\omega$ round the diameter D.

Hence if OX be the axis of resultant momentum; (x, y) the co-ordinates of P relatively to fixed axes OX, OY; θ the inclination of the axis of the ring to O; and ξ the constant value of the resultant momentum: we have

$$\left.\begin{array}{l} \xi\cos\theta = \mathrm{A}u + \mathrm{I};\ -\xi\sin\theta = \mathrm{B}v, \\ \qquad \xi y = \mathfrak{C}\omega; \\ \text{and} \\ \dot{x} = u\cos\theta - v\sin\theta;\ \dot{y} = u\sin\theta + v\cos\theta;\ \dot{\theta} = \omega. \end{array}\right\} (15.)$$

Hence, for θ, we have the differential equation,

$$A\mathfrak{C}\frac{d^2\theta}{dt^2} + \xi\left[I \sin\theta + \frac{A-B}{2B}\xi \sin 2\theta\right] = 0 \quad . \quad (16.)$$

which shows that the ring oscillates rotationally according to the law of a horizontal magnetic needle carrying a bar of soft iron rigidly attached to it parallel to its magnetic axis.

When θ is and remains infinitely small, $\dot{\theta}$, y, and $\dot{y}$ are each infinitely small, $\dot{x}$ remains infinitely nearly constant, and the ring experiences an oscillatory motion in period

$$2\pi\sqrt{\frac{B\mathfrak{C}}{[I + (A - B)\dot{x}](I + A\dot{x})}},$$

compounded of translation along OY and rotation round the diameter D. This result is curiously comparable with the well-known gyroscopic vibrations.

3. Laboratory Notes. By Professor Tait.

1. On Thermo-electricity.

Messrs J. Murray and J. C. Young have been carrying out experimentally the idea mentioned in my former note on this subject. (*Proc.* Dec. 1870.) Their first sets of observations, of the results of which I subjoin a specimen, were made with an iron-silver and an iron-platinum, circuit working opposite ways on a differential galvanometer. The resistances (including the galvanometer coils) were in this particular experiment 53·1 and 25·9 B.A. units respectively, so that but very slight percentage changes could be produced in them by the elevation of temperature of the junctions. As one of a number of closely agreeing preliminary trials the result is extremely satisfactory, though the exact adjustment has not yet been arrived at. To show the parabolas due to the separate circuits, and thus exhibit the advantage of the method, I have requested the experimenters to break the circuits alternately after taking each reading of the complex arrangement, and take a rough reading. The last four columns of the table give the results; but, as the temperatures were probably slightly different from those in the first columns, no very direct comparison can be instituted. A glance at the 4th, 6th, and 8th columns, however, shows how nearly a linear relation between temperature-difference of junctions and galvanometer deflection has been arrived at in the

composite arrangement, while the separate circuits give marked parabolas.

Low Temp.	High Temp.	Pt. Fe., Ag. Fe.	Deflection for increment of 10° C.	Pt. Fe.	Deflection for increment of 10° C.	Ag. Fe.	Deflection for increment of 10° C.
12·3° C	39·0° C	28·5	10·67	44	16·28	17	6·32
„	72	61·5	10·30	96·0	16·08	36	6·03
„	104	93·0	10·14	143·5	15·55	51·5	5·61
„	146·5	136·5	10·17	202·5	15·08	68·0	5·06
12·6	185	172·5	10·0	250·0	14·50	77·0	4·46
„	202·5	190·5	10·03	268·5	14·13	79·5	4·18
12·4	229·5	219·5	10·11	298·5	13·74	81·5	3·74
„	251·5	239·0	10·0	318·0	13·30	81·0	3·38
12·5	263·0	250·5	10·0	330·0	13·16	80·0	3·19
„	272·0	260·0	10·0	337·0	12·98	80·0	3·19

I find great difficulty in obtaining wires of the more infusible metals:—and I am therefore endeavouring to make a complex arrangement for very high temperatures with palladium and two very different kinds of platinum. Wires of nickel, cobalt, molybdenum, rhodium, or iridium, or of any one of these, would be of immense use to me, and I should be happy to hear from any one whether there is a possibility of procuring them.

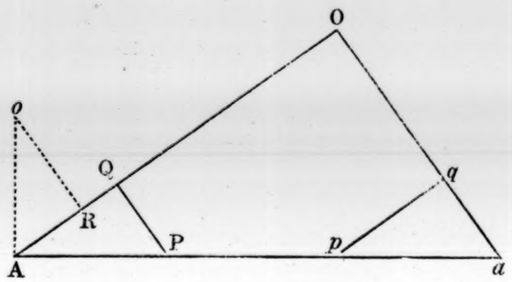
2. On Phyllotaxis.

I was recently led to consider this subject by Professor A. Dickson, who showed me some of his beautifully-mounted specimens, and explained to me the method he employs for the determination of the divergence, and of the successive leaves of the fundamental spiral or spirals. He referred me to two terribly elaborate papers by Bravais,* and I have since met with another of a similar character by Naumann.† These papers certainly cannot be supposed to present the subject from the simplest point of view. I do not doubt that the results I have here arrived at are to be found in some form or other in their pages, which are announced as completely elucidating the question; but I have not sought for them, my sole object having been to put what seem to me the elements of the matter as simply and intelligibly as I could.

* Annales des Sciences Naturelles, 1839.

† Poggendorff's Annalen, 1842.

Let A, a, represent the same leaf in a plane development of a branch or fir-cone (regarded as cylindrical); O, a leaf which can be reached from A by m steps in a right-handed spiral, developed into the straight line AO, and by n steps from a in a left-handed spiral aO. These spirals may in general be chosen so that m and n are not large numbers (3, 5, 8, 13, &c., being very common values); but they *must* (and can always) be so taken that m spirals parallel to aO, and n parallel to AO, shall separately include all the leaves on the stem or cone.



If m and n have a common factor λ, there will be $\lambda - 1$ leaves (besides A) which are situated *exactly* on the line Aa, and therefore the arrangement is composite, or has λ distinct fundamental spirals. If m' and n' be the quotients of m and n by λ, they are to be treated as m and n are treated below; and this case thus merges into the simpler one, so that we need not allude to it again.

It is obvious that, in seeking the fundamental spiral, we must choose the leaf *nearest* to Aa on the side towards O, as that succeeding A or a. The fundamental spiral will thus be right-handed if P, which is nearer to A than to a, be this leaf—left-handed if it be p. Of course, we may have a left-handed fundamental spiral in the former case, and a right-handed one in the latter; but the divergence in either will be greater than two right angles, and this the majority of botanists seem to avoid.

Draw PQ and pq respectively parallel to aO and AO, then the requisite condition is that

$$\frac{n}{m}\text{AQ} - \text{PQ}, \quad \text{or } \frac{m}{n} aq - pq,$$

shall be as small as possible.

Hence, if $\frac{\mu}{\nu}$ be the last convergent to $\frac{m}{n}$, and if $\frac{\mu}{\nu} > \frac{m}{n}$, it is

obvious that to get at P we must count μ leaves along AQ, and ν along QP. If, however, $\frac{\mu}{\nu} \prec \frac{m}{n}$, count ν leaves along aq, and μ along qp. P, or p, thus found is the next leaf of the fundamental spiral to A or a; the next is derived from it by a second application of the same process, and so on.

There is no necessity for restricting the development, as given above, to *once* round the cone. Suppose we go several times round and that A, a, α, &c., are successive positions of the same leaf. The processes given above may be employed, and the results will be of the same nature. But this extension enables us to obtain (more and more approximately, sometimes accurately) a *right* angle aAo, where o is a leaf reached after several turns of the fundamental spiral. This indicates that the leaves may be grouped (approximately or accurately) in lines parallel to the axis of the stem or cone. When this can be done accurately, it is easy to see that (since one of $\frac{n}{\nu}$, $\frac{m}{\mu}$, is greater, and the other less, than the number of leaves in one turn of the fundamental spiral) the difference of azimuth of two successive leaves of that spiral must be expressible in the form

$$2\pi \frac{r\mu + s\nu}{rm + sn};$$

where s and r are necessarily very small positive integers in all the ordinary cases of phyllotaxis, since they are the numbers of leaves in AR, Ro, respectively, which are portions of the spirals on which or parallel to which, m and n were measured.

The fraction

$$\frac{r\mu + s\nu}{rm + sn}$$

has been called the *divergence* of the fundamental spiral. Of its constituents the numbers m, n, r, s are at once given by inspection of any cone or stem, and (from m and n) μ and ν are easily calculated.

To extend this investigation to the cases in which the divergence is altered by torsion of the cone, it is merely necessary to notice that such a process alters only r and s. It produces, in fact, a simple shear in the developed figure.

Added, March 20th, 1871, *in consequence of some remarks made by Professor Dickson at the Meeting of that date.*

It is obvious that if the same leaf, O, be reached from A by m steps of a right-handed, and n of a left-handed, spiral (such that n of the former and m of the latter contain, severally, all the leaves), another common leaf can be reached by $m-n$ steps of the right-handed spiral, and n steps of a new left-handed one (these spirals possessing the same property of severally containing, in groups of n and $m-n$ respectively, all the leaves). This process may be carried on, when m and n are prime to one another, until we have steps represented by 1 and 1, in which case we obviously arrive at the leaf of the fundamental spiral next to A. It is better, however, to carry the process only the length of steps 1 and t, where t is determined by the condition that 1 and $t+1$ would give spirals both right-handed or both left-handed.

Now, in the majority of cases of fir-cones, it seems that we have t, found in this way, $=2$, i.e., *there are less than three leaves in a single turn of the fundamental spiral.* It is of course obvious that there can never be less than two, and the case of exactly two corresponds to the simplest of all possible arrangements, that in which the leaves are placed alternately on opposite sides of the stem. Fir-cones, therefore, give in general the arrangement next to this in order of simplicity. Hence, for such cones, and for all other leaf arrangements which are based on the same elementary condition, the values of m and n for the most conspicuous spirals must be of the forms

$$\begin{array}{ccccc} 2, & 3, & 5, & 8, & \text{\&c.,} \\ 1, & 2, & 3, & 5, & \text{\&c.} \end{array}$$

These simple considerations explain completely the so-called mysterious appearance of terms of the recurring series 1, 2, 3, 5, 8, 13, &c., &c. The other natural series, usually but misleadingly represented by convergents to an infinitely extended continued fraction, are easily explained as above by taking $t=3$, 4, &c., &c. As a purely mathematical question it is interesting to verify the consistency of the statements just made, where the change in t is introduced, with those above made as to the effects of torsion in altering r and s. But this may easily be supplied by any reader who possesses a small knowledge of algebra.

Monday, 6th March 1871.

Dr CHRISTISON, President, in the Chair.

The following Communications were read:—

1. Account of the Extension of the Seven-Place Logarithmic Tables, from 100,000 to 200,000. By Edward Sang, Esq.

Abstract.

In this paper the details were given of the computations made for extending the Table of Seven-Place Logarithms to 200,000 and of the precautions taken to ensure accuracy in the printed work.

The calculations were originally intended for a Nine-Place Table to One Million; and the manuscript shows the logarithms to fifteen places, with their first and second differences for all numbers from 100,000 to 200,000.

2. On the Place and Power of Accent in Language. By Professor Blackie.

Professor Blackie then read a paper on "The Place and Power of Accent in Language." On the subject of accent and quantity, he remarked, especially in relation to the learned languages, the greatest confusion had prevailed, and the existing practice was altogether unreasonable and anomalous. In articulate sound four things had to be distinguished—volume or bulk, force or emphasis, elevation and depression, and prolongation or duration. English scholars had shown an unhappy incapacity of not being able to distinguish between stress and prolongation, and thus had been led to introduce the general practice of pronouncing Greek with Latin accents. In laying down the principles by which syllabic accentuation is guided, four points are to be attended to—significance, euphony, variety, and convenience. Fashion, of course, and custom have wide sway in this domain; but in the original structure

of language we have to look to significance and euphony rather than arbitrary usage, as the main causes which determined the place of the accent. In compound words it was natural that the qualifying or contrasting element should be emphasised, as in the proper Scotch pronunciation of *Balfour* (Coldtown), where the accent lies on that element of the word which distinguishes it from other *Bals* or towns. As to euphony, those languages are least euphonious which, like English and Gaelic, have a preference for the ante-penultimate accent, while those are most euphonious which, like Latin, Greek, and Italian, abound in penultimate or ultimate accented syllables. In respect of euphony, as well as variety, the Greek language was superior to the Latin, in that it allowed the accent on any of the three last places, while Latin allowed it only on the penult and ante-penult. The attempt to make out a special and exceptional case for Greek accents were vain. It is perfectly clear from the statements of the ancient Greek grammarians, that the Greek acute accent consisted not only in the raising of the voice on the syllable, as Professor Munro imagines, but in a greater emphasis or stress. The prejudice which has so long existed against the use of Greek accents arose partly from mere carelessness, partly from a notion that the observance of the accent would interfere with the proper quantity of the vowels, and destroy the beauty of classical verse. But this notion is altogether unfounded, as classical verse, originally an inseparable part of musical science, was not governed in any respect by the spoken accent, but guided entirely by the rhythmical ictus or time-beat. Practically, there was no difficulty in reading Greek prose by the accent, and Greek poetry by the quantity. In the μέλος, or purely musical part of the drama, the spoken accent naturally fell away. In recitation a sort of compromise probably took place, which is perfectly easy of execution. The paper included a history or review of the doctrines of learned men and great scholars on the subject of Greek accentuation, from Erasmus down to Chandler, Munro, Clark, and Geldart. It was astonishing that such confusion and beating the air about imaginary difficulties should have so long prevailed on a matter comparatively so simple; but there was not the slightest doubt that the moment our classical teachers should recur to living nature, instead of being governed by dead tradition in this

matter, the present monstrous, pernicious, and perplexing practice of reading Greek with Latin accentuation must cease. Independent of its absurdity, the loss of time occasioned by teaching one accent to the ear, and another to the understanding, should be motive enough for all teachers to deliver our classical schools from a yoke which, originally imposed by sheer laziness, is now supported only by ignorance, prejudice, and the tyranny of custom.

Monday, 20*th March* 1871.

D. MILNE HOME, LL.D., Vice-President, in the Chair.

The following Communications were read :—

1. Notice of Exhibition of Vegetable Spirals. By Professor Alexander Dickson.

Dr Dickson exhibited a number of specimens, chiefly Fir Cones and Cacti, illustrating the principal series of vegetable spirals. Almost all the cacti and many of the cones were from the Edinburgh Botanic Garden and the Museum of Economic Botany there. As the nomenclature of the cacti in the Edinburgh garden, as in many other botanic gardens, is in a state of considerable confusion, the specific names will not be referred to, and the generic ones, even, must in some cases be held as only approximately correct. This, however, is of the less consequence as the phyllotaxis of such plants is eminently variable even in the same species. Ten different series or systems of spirals were illustrated by specimens, of which the following may be noted.

I. Ordinary series, $\frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{8}, \frac{5}{13}$, &c.

Cones of *Abies Douglasii* $\left(\frac{5}{13}\right)$: *A. excelsa* $\left(\frac{8}{21}\right)$: *Pinus Coulteri* $\left(\frac{13}{34}\right)$: *Araucaria excelsa* $\left(\frac{21}{55}\right)$: *Araucaria imbricata* $\left(\frac{34}{89}\right)$: Bijugates of the same series in cone of *Abies Douglasii* $\left(\frac{2}{5\times 2}\right)$, the solitary abnormality out of

200 cones examined; in an Echinocactus $\left(\frac{3}{8\times2}\right)$; and in *Abies excelsa* and *Pinus Pinaster* $\left(\frac{8}{21\times2}\right)$. Trijugates in an Echinocactus $\left(\frac{2}{5\times3}\right)$; and in cones of *Abies excelsa* and *Pinus Pinaster* $\left(\frac{5}{13\times3}\right)$.

II. Series, $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}$, &c.

Cones of *Pinus Pinaster, P. Lambertiana*, and *Abies excelsa* $\left(\frac{5}{18}\right)$: Mammillaria $\left(\frac{13}{47}\right)$: cone of *Pinus Jeffreyi* $\left(\frac{21}{76.}\right)$. Bijugates of same series in an Echinocactus $\left(\frac{2}{7\times2}\right)$; and one shoot of another Echinocactus $\left(\frac{3}{11\times2}\right)$.

III. Series, $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}$, &c.

Echinocactus $\left(\frac{2}{9}\right)$; cone of *Pinus Pinaster* $\left(\frac{5}{23}\right.$ or possibly $\left.\frac{8}{37}\right)$. Bijugate of same series in an Echinocactus $\left(\frac{2}{9\times2}\right)$.

IV. Series, $\frac{1}{5}, \frac{1}{6}, \frac{2}{11}, \frac{3}{17}$, &c.

Two Echinocacti $\left(\frac{2}{11}\right)$.

V. Series, $\frac{1}{6}, \frac{1}{7}, \frac{2}{13}, \frac{3}{20}$, &c.

A Cereus? and Mammillaria? $\left(\frac{2}{13}\right)$.

VI. Series, $\frac{1}{7}, \frac{1}{8}, \frac{2}{15}, \frac{3}{23}$, &c.

Melocactus and Echinocactus $\left(\frac{2}{15}\right)$.

VII. Series, $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}$, &c.

Echinocactus? $\left(\frac{5}{12}\right)$. Bijugate of same series in the middle region of a cone of *Pinus Lambertiana* in the Museum, Edinburgh Botanic Garden $\left(\frac{5}{12\times2}\right)$; the two parallel spirals,

here, ran to the right hand, while the single spiral at top and bottom of the cone $\left(\frac{5}{23}\right)$ was left-handed.

VIII. Series, $\frac{1}{2}, \frac{3}{7}, \frac{4}{9}, \frac{7}{16}$. &c.

Echinocactus $\left(\frac{7}{16}\right)$.

IX. Series, $\frac{1}{3}, \frac{2}{7}, \frac{3}{10}, \frac{5}{17}$, &c.

Echinocactus $\left(\frac{5}{17}\right)$.

X. Series, $\frac{1}{4}, \frac{2}{9}, \frac{3}{13}, \frac{5}{22}$, &c.

Cone of *Pinus Pinaster*, in Museum of Edinburgh Botanic Garden, $\left(\frac{5}{22}\right)$.

Dr Dickson drew special attention to five flower spikes of *Banksia occidentalis*, which he had examined from the Edinburgh Botanic Garden. These he found to exhibit four distinct arrangements. One had fourteen vertical rows of bracts, from alternate whorls of seven; two presented thirteen verticals, from a $\frac{2}{13}$ arrangement; one had also thirteen verticals, but from a $\frac{5}{13}$ arrangement; the fifth had twelve verticals, from a $\frac{5}{12}$ arrangement.

2. On the Old River Terraces of the Spey, viewed in connection with certain proofs of the Antiquity of Man. By the Rev. Thomas Brown, F.R.S.E.

Abstract.

The author referred to the paper which he had read on the terraces of the Earn and Teith,* and then described similar deposits which he had observed last autumn on the Spey, giving examples with drawings, from the neighbourhood of Kingussie, Dalvey, and Ballindalloch. The arguments formerly adduced† were equally con-

* Trans. Roy. Soc. Ed. xxvi. 149. † Ibid. 154–163.

clusive in the Spey to show that these terraces were not old sea beaches nor lake margins, but the fluviatile deposits of some former epoch when the floods rose to a greater height. The problem then came to be, In what way are we to explain the action of the river in throwing up deposits 60, 80 feet, or even more above its bed? There are two ways, in one or other of which this may be accounted for,—either by supposing the river bed to have lain on its present level, and allowing rainfall sufficient to flood the channels up to the requisite height; or by supposing the bed of the stream to have been formerly at a higher level, and that, after forming the terraces, the current had excavated its bed down to where it now is. It is the second of these views which has found most favour among geologists, and various suggestions have been offered as to how the bed of the stream was formerly elevated.

One explanation is, that at the time of the highest terrace, the line of the valley, then comparatively shallow, was occupied by the original rock, still to a great extent *in situ.* In regard to our Scottish valleys this explanation is inadmissible. It was formerly shown, from the position of the boulder clay,* that the rocky structure of these river-courses had been hollowed out nearly as deep as now previously to the formation of the terraces; but apart from the Boulder clay the terraces themselves, as will be shown, prove the same thing, for example, the 70 feet terrace at Kingussie.

Another explanation is, that during the last submergence of Scotland the valleys had been filled by marine gravels, &c., and that the river bed had been thus lifted to the requisite height. This view, however, must also be set aside, because after that submergence, the valleys of Scotland were occupied by glaciers, which must to a great extent have cleared out these previous marine deposits.† Especially must this have taken place in Strathspey, lying so high above the sea, and connected with the central mountain-masses of the country. The glacier must have ploughed out the marine debris. It was after that the terraces were formed.

There is a third suggestion, that the river had raised itself on its own alluvium, formed the terraces, and then re-excavated its

* Trans. Roy. Soc. Ed., vol. xxvi., 171.

† Sir C. Lyell's Antiquity of Man, p. 206. Scenery of Scotland, by Mr Geikie, p. 347

bed. But here, again, the objections are equally decisive. ***First***, the raising of a river bed in this way seems to take place only when the current has reached some comparatively level part of its course, as in the Po or Nile. The Spey is remarkable for the steep incline of its bed. The Ordnance Survey * shows that for nearly 30 miles below Grantown it goes down more than 600 feet,—fully 20 feet a mile. The current is strong, the old terraces are high. The idea is not for a moment to be thought of that it could have acted as the sluggish rivers which silt up their beds. But, *secondly*, how did the river, after silting up its bed, and raising itself, come to change its action, and cut its way down? Is any such case on record applicable to any river course as a whole? If such a revolution of river action be exceptional, or if it be unknown in nature, we should surely not be warranted in applying it to the rivers of Scotland generally at the period of the terraces.

Thus the idea that the river bed had formerly been elevated is encompassed by difficulties. In whatever form the explanation is put, objections at once suggest themselves which would appear to be fatal.

Turning to the other view, that the river had flowed on its present level, we find that the one great difficulty is the vast amount of water which would be needed to flood the channels up to the requisite height. Mr Prestwich, referring to the Somme and some English rivers, has calculated that it would require 500 times the present flow of the stream to form the 80 feet terrace.† When we look closely into the matter, however, this difficulty diminishes. The result of 500 : 1 is obtained by taking the present flow of the Somme at 800 square feet sectional area. That represents the river when *not* in flood. As the 80 feet terrace, however, is admittedly the work of the old river when *in* flood, we must take the present Somme also in flood, and that is not 800 but 3000 square feet (Prestwich).‡ The effect of this first correction is to bring the 500 : 1 down to 133 : 1. But, further, when Mr Prestwich comes to put all the facts together, he estimates the old Somme at a little more than five times the present—16,000§ against 3000 of

* As yet unpublished; but these results were obligingly communicated by Col. Sir H. James, F.R.S.

† Phil. Trans., vol. cliv., p. 265. ‡ Ibid., 292. § Ibid.

sectional area—and the result is, that if we compare his own view with that which he ascribes to his opponents, the 133 : 1 is further diminished to 25 : 1. But there is a still more important fact to be taken into account. In calculating the sectional area of the old river the whole valley is assumed as empty; but this it cannot have been, at least here in Scotland. If the rocky structure of the valleys was excavated, and the rock removed, how shall the floods be raised high enough to form the terraces? There only remain water and alluvium to fill the space. The only reasonable view is that the area of the valley was to a large extent occupied by masses of alluvium since removed. And this is borne out by what we actually find—fragments of old gravelly platforms left standing to tell of deposits which evidently were at one time far more extended. A third correction, not less important than the others, must be on this ground applied to Mr Prestwich's calculation. So far from the valley having been empty, it must to a great extent have been filled with alluvial deposit since denuded. The difficulty raised as to the volume of the old floods is thus to a great extent set aside.

At various points along the Spey—Kingussie, Coulnakyle, Cromdale—transverse sections of the valley were given, showing the height of the terraces. From the width of the valley in these cases (of which details were given) it appeared that a calculation like that of Mr Prestwich in the Somme would bring out results equally incredible as to the old floods, but owing to the above corrections this difficulty is removed, and the remarkable thing is that the 70 feet terrace at Kingussie has been laid open in an old river course, and the 80 feet terrace at Cromdale in a railway cutting so as to bring out similar results to those formerly shown from the valley of Monzie.* Explain the matter how we may, the river, with an open valley three-fourths of a mile wide, has begun at the bottom, on the level of its present bed, and piled up these deposits to the height of 70 or 80 feet. That they are the work of the river is proved by the way in which the platform-like surface of the terrace slopes down the stream.

The idea of ascribing these high-lying terraces simply to the greater flooding power of some former time was suggested by a comparison between the deposits of the Ruchil with those of the

* Trans. Roy. Soc. Ed., vol. xxvi. pp. 171, 172.

Upper Earn, and of the terraces of Loch Lubnaig with those of Loch Earn, as formerly explained.* It is confirmed by the terraces of the Spey, and more especially by the failure of all the other explanations.

Our knowledge of this whole series of deposits is as yet far too imperfect to allow of anything like a complete theory of their formation. If a suggestion might be offered, perhaps the course of events may have been something like this. When the glacial epoch ended, and the covering of ice and snow melted off Scotland, there would be no small amount of debris over the face of the country, and, unprotected by vegetable covering, it would be washed down into the valleys. Every one admits that the rivers of that age were larger than now—how much larger it is difficult to say. If the Spey had five times its present volume (as Mr Prestwich suggests in the case of the Somme) it would, judging from the present force of its current, assuredly keep its central channel open whatever the amount of debris which came down into the valley. River-like, it would form its banks, and spread out its haughs up to the height to which its floods could rise, *when confined to its comparatively narrow channel.* In the case supposed that height may have been great; and these old high terraces may be the fragments of alluvial platforms, which once spread out along the valley, where the old floods had raised them. Before the whole facts are fully explained, it seems probable that our ideas of the amount of water present in these old floods may have to be enlarged.

The bearing of these facts on certain arguments for the antiquity of man was considered, with special reference to the Spey deposits. There are gravel beds along the Somme in France, which, up to the height of 80 feet, contain flint weapons, which are held to be of human manufacture; and the argument is, that the river has excavated through the rock the valley in which it now flows—that this has been done since the deposition of the gravels, and to allow time for such excavation their age, and consequently the human period, must be carried back into some vast antiquity.

But here is an important fact, which the deposits of the Spey make still more clear in some respects than those of the Earn and

* Trans. Royal Soc. Edin., vol. xxvi, 163-166.

Teith. Along our Scottish rivers there are similar high gravels, 80 feet or more above the stream; and it is known that, previously to the time of their formation, the rocky structure of our valleys had already been hollowed out nearly as deep as now. This is shown at Kingussie, where the 70 feet terrace—and at Cromdale, where the 80 feet terrace—are seen resting on the rock nearly on a level with the river-bed. If, then, with the rocky bed down on its present level, the Scottish streams have managed *somehow* to form those high-lying deposits, why may not the French rivers have done the same? In that case, the Somme would require no time for the subsequent excavation of its valley, and the human period, so far as this argument is concerned, may not be so long after all.

The force of this does not depend on the correctness of the views stated above as to the formation of these terraces. *Whatever* was the way in which the Scottish rivers went to work, it was after the rock had been excavated, and the question would still be, why may not the French rivers have done the same?

One point seems clear, that the case of the French gravels must be shown to differ from those of Scotland before the advocates of extreme antiquity can prove their case from the Somme. After admitting the case in Scotland, if a distinction is to be made in regard to France, the burden of proof will lie with them. The probabilities would certainly seem to be against them. Rivers and valleys have the same laws in different countries. If the French rivers be alleged to have acted differently from the Scottish it may have been so, but the grounds of the difference would need to be adequate, and the proof clear. In the present case, the alleged distinction has reference altogether to the excavation of the rock. In France, they say it had to be done subsequently to the time of the terraces; in Scotland, it must be admitted to have been done before. Are there any grounds on which such a distinction can be made good? Was there such a difference in the formation of valleys between Scotland and France?

It will not be alleged that the soft texture of the chalk rock of the Somme, as contrasted with our harder rocks, can form the ground of distinction. In France itself the same valley-systems traverse many different kinds of rock.

Nor can it be said that the submergence of Scotland as contrasted with the area of the Somme, which was not submerged, can constitute the difference, for Mr Prestwich has shown* not only that the French system of valleys has crossed into the south of England, but that it prevails indifferently as much beyond as within the line of submergence traced by Sir C. Lyell. That submergence seems in this respect to make no difference.

It is equally in vain to allege that the large amount of alluvium in the Scottish valleys makes such a ground of distinction when contrasted with the lesser amount of such deposits on the Somme. The alluvium along our Scottish streams is a very variable quantity as between valley and valley, and as between different portions of the same valley. On the other hand, the amount of the Somme gravels at Amiens and above it, is great—so great, that both Mr Prestwich and Sir Charles Lyell argue in favour of their antiquity, from the length of time which must have been needed to accumulate such a volume of debris.† On the Oise also, and some neighbouring streams, the amount of alluvium is described as very great.

It is enough, however, to remark, that the burden of proof lies with the advocates of antiquity, and that its difficulties have not been surmounted. On the other hand, there is one thing which they may fairly be asked to do—if they maintain that the French and Scottish valleys have been formed on different principles—to show where the two systems meet. The French method, as we have seen, crosses into England. No one will maintain that the Scottish stops at the Tweed. Somewhere they must come in contact. It would be instructive if some one would try to show us two conterminous valleys wrought on the opposite plans. The attempt would probably evince the impossibility of drawing such a distinction. In all that is important, the French and Scottish valley systems go together.

The whole of these remarks are submitted as suggestions, showing the need of much more complete investigation. On this whole series of deposits we have much to learn,—far too much to admit of anything like confident conclusions being drawn as yet. The only safe course is to await the results of future research.

* Phil. Trans., vol. cliv. Pl. iv.

† Prestwich, *ut sup*, 286. Sir C. Lyell, "Antiq. of Man," p. 144.

If difficulty be still felt in regard to the amount of water required for those old floods, we might appeal to the kind of proof by which the existence of a former glacial epoch in Scotland is established. Who that looked to the present ice and snow of a Scottish winter, could think it likely that glaciers once filled the valleys of the Pentlands, and that masses of moving ice rose over the flanks of Arthur's Seat. We point to the rounded and striated rocks, and say, there are the foot-prints of the old glacier,—and the thing is proved, no matter how different may be the cold of our present winters. And why not reason thus in regard to the old floods? Who that looks on the present flow of our streams could realise floods able to raise those old 80 feet terraces? But why should we not point to these deposits where they lie, and say, these stratified gravels and bedded sands are the workmanship of the old currents, which once swept and eddied at that height down these valleys. If this kind of evidence makes you believe in the great old glacier all unlike our present ice, why should not similar proof make you believe in the great old floods of a former epoch, all unlike though they may be to our present streams?

And yet in Strathspey, with the traces of the Moray floods all around us, it is easier to believe these things than it would be almost anywhere else. It was at Coulnakyle, the scene of one of these drawings, that Captain M'Donald, R.N., a sailor of the old school, looked out and saw the Spey, about a mile wide, covered with waves, that put him in mind of Spithead in a fresh gale, and felt sure, as he told Sir T. D. Lauder, that he could have sailed a fifty-gun ship from Boat of Garten to Bellifurth, a distance of seven miles. The small burn of Drumlochan, which in its ordinary state "is hardly sufficient to keep the saw-mill going," rose till it swept away two bridges of twenty feet span, the column of water being estimated at 400 square feet sectional area. As the miller of Dalnabo expressed it, "the height the burns rose to that day was just a' thegither ridiculous." In looking back to the time of these old deposits, it is generally admitted that the volume of the rivers was decidedly greater than it is now. Mr Prestwich, as we have seen, assumes that the old Somme was five times the present. If we might suppose something like this in the Spey—if, further, there was along the valley an amount of alluvium sufficient to confine

the stream to its own channel—and if, from whatever cause, there came floods which would do in proportion for the enlarged Spey what the floods of 1829 did for the Drumlochan Burn, it does not appear as if the solution of the problem as to the formation of these high terraces should be difficult. It is in this direction that the solution is to be sought.

Monday, 3d April 1871.

Professor KELLAND in the Chair.

The following Communications were read:—

1. On the Gravid Uterus and the Arrangement of the Fœtal Membranes in the Cetacea. By Professor Turner.

(*Abstract.*)

In this memoir the author described the dissection of the gravid uterus of an Orca gladiator, for which he was indebted to Mr James Gatherer of Lerwick. The paper contained an account of the uterus and appendages, the fœtal membranes, the position and general form of the fœtus, and a comparison of the placentation with that of other mammals possessing the diffused form of placenta. The structure of the uterine mucous membrane, its subdivision into a gland layer and a crypt layer, the relations of the glands to the crypts, their structure, the arrangement of their blood-vessels, and the much greater vascularity of the crypts than of the glands, were especially described. The chorion, though with diffused villi, possessed not only a small non-villous part at each pole, but a third larger bare spot opposite the os uteri internum; the non-villous spots corresponded, therefore, to the three uterine orifices. The arrangement and structure of the villi, the relations of the vessels to them and to the chorion generally were described; the plexus of capillaries within the villi became continuous with a network, termed sub-chorionic, situated immediately beneath the intervillous part of the chorion, from this latter plexus the rootlets of the umbilical vein arose. The intra-villous capillary plexus lay in relation to the system of capillaries situated in the walls of the uterine

crypts, whilst the sub-chorionic lay in relation to the capillaries situated beneath the plane of the general uterine mucous surface. The amnion formed a continuous bag from one horn of the chorion to the other, but did not reach the poles of the latter. In the left horn, which contained the fœtus, it extended to 2 inches, in the right to 9 inches from the corresponding pole of the chorion, its free surface was studded with small pedunculated corpuscles. The allantois was not so extensive as the amnion. The urachus expanded into a large funnel-shaped sac, which bifurcated when it reached the chorion and formed a right and left cylindrical horn; the left reached to 7 inches from the left pole of the chorion, the right to 21 inches from the right pole.

2. Note on some Anomalous Spectra. By H. F. Talbot.

A recent number of Poggendorff's "Annalen" contains a short but interesting paper by Christiansen, of Copenhagen, in which he states that a hollow prism filled with the alcoholic solution of fuchsine produces a highly anomalous spectrum, which, instead of proceeding regularly from the red to the violet like the ordinary solar spectrum, stops at a certain point, returns backward, then stops again and resumes a direct course to the end. This paper by Christiansen, kindly pointed out to me by Professor Tait, recalls to my memory an experiment which I formerly made more than thirty years ago, and which, with the permission of the Society, I will briefly describe, premising, however, that I write from memory, and without access at present to the original paper which I believe I have still preserved. My account may therefore contain some inaccuracies, but the general nature of the experiment was as follows:—I prepared some square pieces of window glass, about an inch square. Taking one of these, I placed upon it a drop of a strong solution of some salt of chromium, which, if I remember rightly, was the double oxalate of chromium and potash, but it may have been that substance more or less modified. By placing a second square of glass on the first, the drop was spread out in a thin film, but it was prevented from becoming too thin by four pellets of wax placed at the corners of the square, which likewise served to hold the two pieces of glass together. The glasses were then laid aside for some hours

until crystals formed in the liquid. These were necessarily thin, since their thickness was limited by the interval between the glasses. Of course the central part of each crystal, except the smallest ones, was bounded by parallel planes, but the extremities were bevilled at various angles, forming so many little prisms, the smallest of them floating in the liquid. When a distant candle was viewed through these glasses, having the little prisms interposed, a great number of spectra became visible, caused by the inclined edges. Most of these were no doubt very imperfect, but by trying the glass at various points, some very distinct spectra were met with, and these could with some trouble be isolated by covering the glass with a card pierced with a pin-hole. It was then seen that each prism (or oblique edge of crystal) produced two spectra oppositely polarised and widely separated. One of these spectra was normal; there was nothing particular about it. The colours of the other were very anomalous, and, after many experiments, I came to the conclusion that they could only be explained by the supposition that the spectrum, after proceeding for a certain distance, stopped short and returned upon itself.

No accurate measurements, however, were made, because it always happened that, after the lapse of a minute or two, the crystals dissolved in the surrounding liquid, owing to the warmth of the hand or eye. The presence of the liquid, however, was necessary to give the crystals the requisite transparency, and, moreover, the liquid virtually diminishes the angle of the prism floating in it, which otherwise would be too great to give a good result. I never published this experiment, because I found it delicate and capricious, and I was reluctant to publish any facts that might be difficult for others to verify. But I have several times described it to Sir D. Brewster in conversation, and he always said that he thought it very important, at the same time suggesting that there might perhaps be some fallacy. This was because he doubted the possibility of a spectrum being partially inverted or returning on itself. But this doubt seems now to be wholly removed by Christiansen's experiment, in which there seem to be two inversions in the spectrum, and therefore I no longer hesitate to state the grounds on which I concluded long ago that this phenomenon was possible.

Writing entirely from memory, it is possible that I may have fallen into some inaccuracies in this brief account, which, if it should be the case, I trust the Society will, under the circumstance, kindly excuse.

P.S.—Since the above remarks were written, the first number of Poggendorff's "Annalen" for the present year has been received in Edinburgh. This contains a long article by Kundt on the subject of Christiansen's experiment.

He finds that anomalous spectra are given by all the aniline colours, and by permanganate of potash. Such spectra turn back upon themselves, generally having the green at one extremity, the blue being situated between the green and the red.

Hence this property is possessed by an extensive class of bodies, and must form a new and separate branch of optics. He says that the phenomenon only occurs when a very strong solution of the substance is employed in the form of a liquid prism of 25°. But only the thin extreme edge of the prism is available, the thickness of the rest rendering it opaque. He failed in the attempt to form a solid prism by mixing collodion with the alcoholic solution, but this might perhaps be achieved by other means. In the meantime a wide field of experiment is open.

3. Laboratory Notes. By Professor Tait.

1. On Anomalous Spectra, and on a simple Direct-vision Spectroscope.

When I first saw Le Roux's account of his very singular discovery of the abnormal refraction of iodine vapour, I was inclined to attribute the phenomenon to something similar to over-correction of an achromatic combination. In fact, if a hollow prism be filled with a mixture of two gases or vapours, one of which is more refractive than air, the other less refractive; while the second body is more dispersive than the first; it is easy to see that Le Roux's result might be obtained, although each of the substances employed is free from anomalous refractive properties. In a recent conversation with Mr Talbot, I happened to mention the subject, and I learned from him his remarkable observation just laid before the Society. I have since, when I had an opportunity,

made several trials with hollow prisms and prismatic vessels, using various substances, such as oils of cassia and turpentine, toluol, alcohol, saturated solutions of salts, &c., with the view of imitating, with nearly transparent substances, the singular results obtained by Talbot, Christiansen, and Kundt. The observations are certainly very easy in one sense, though very laborious in fact; but I have already produced a spectrum doubled on itself, and have no doubt that with patience I shall be able to produce one with two and even more inversions; though, of course, the more numerous are the inversions the smaller is the scale of the whole phenomenon. The easiest method seems to be to put into a hollow prism a mixture of two substances of very different refractive powers, and to immerse it in a prism or trough containing a substance of intermediate refractive power. When a trough is employed, an external glass prism may with advantage be used along with the combination. The sought phenomenon is, of course, obtained best near the point of adjustment for achromatism, and is in fact very closely connected with the investigations of Dr Blair in his attempts to improve the achromatic telescope by using fluid lenses.

One of my hastily set-up combinations (of two liquids only) gave me a direct-vision spectroscope complete, more powerful than one of Browning's excellent instruments with five glass prisms, and I have little doubt that in this way very good results may be obtained. But, if it be needful to examine only a small region of the spectrum at a time, practically unlimited dispersion may be obtained by using so very simple a combination as two approximately isosceles flint prisms of small angle with their edges together and their adjacent faces inclined at an angle approaching to 180°, so as to form a hollow prism to be filled with oil of cassia. In fact, the dispersion is in this case easily seen to be nearly proportional to the tangent of half the angle of the oil prism. If two kinds of glass, of very different dispersive powers, but of nearly equal mean refractive powers, could be obtained, a permanent combination might be easily formed on this plan, giving as much dispersion as a very long train of ordinary prisms, and losing scarcely any light. A slight inclination of the ends to one another will enable us to use ordinary flint and crown for the purpose, except in so far as total reflection may interfere. Such a combination, adjusted for the red

ray C, seems to promise to be of considerable use in observations of the sun's atmosphere. A somewhat similar result may be obtained by using a single large prism, one of whose faces, employed for total reflection, has a very slight cylindrical curvature.

2. On a Method of illustrating to a large Audience the Composition of simple Harmonic Motions under various conditions.

I have often felt the difficulty of illustrating, by means of Airy's Wave Machine, and various other complex instruments of a similar character, the composition of plane polarised rays into a single elliptically or circularly polarised one; the difficulty arising chiefly in showing separately, but in close succession, to the audience the two vibrations which are to be compounded, and their resultant. Lissajoux's apparatus would exactly answer the purpose if we had tuning-forks vibrating 10 or 15 times a second, its sole defect being the extreme rapidity with which differences of phase are run through; and, in fact, I have tried metronome pendulums with mirrors attached to them; but I have since found the following arrangement to be much more satisfactory. It consists simply in using plane mirrors rotating about axes very nearly perpendicular to their surfaces. A ray reflected almost normally from each of two such mirrors, equally inclined to their axes, and rotating in opposite directions with equal angular velocities, has communicated to it a simple harmonic vibration, whose line and phase can be adjusted at pleasure by a touch. Two such systems of pairs of mirrors, connected by elastic bands with an axle driven by hand, enable the operator to illustrate every combination of two simple-harmonic motions, as well as of circular and elliptic vibrations. By an obvious adjustment it is easy to use, instead of equal periods of vibration, periods bearing any desired relation to one another; and by crossing one or more of the bands we reverse the direction of rotation in the corresponding shafts. It is absolutely necessary to have adjusting screws by which to regulate the inclination of each mirror to its axis.

3. On a simple Mode of explaining the Optical Effects of Mirrors and Lenses.

It is very singular to notice how small a matter makes the difference between the intelligibility and unintelligibility of a demon-

stration to an audience as a whole not mathematical. In no part of Physics have I found this so marked as in the most elementary portions of geometrical optics. Such a formula as

$$\frac{1}{u}+\frac{1}{v}=\frac{2}{r},$$

when interpreted directly as signifying that "the sum of the reciprocals of the distances of the object and image from the surface of a concave spherical mirror, is equal to double the reciprocal of the radius of the mirror," if understood at all, is understood as a sort of *memoria technica* which enables the student to make calculations; but unless he have some knowledge of mathematics it suggests absolutely no higher meaning. If, however, we give to the various terms of the formula their meanings in terms of the *divergence* of the incident and reflected beams, and of the normals to the reflecting surface, even the non-mathematical student easily understands the relation signified. I am indebted to Mr Sang for a reference to Lloyd *On Light and Vision*, 1831, in which this mode of presenting the subject is introduced, but I think the term "vergency" there used is hardly so convenient as the more commonly employed word divergence. Our fundamental optical fact is that to produce the most distinct vision rays must diverge as if from a point about ten inches from the eye. No one has any difficulty in understanding this. As my object has been merely to mention to the Society what I have found to be a method (however trivial in itself, yet) of really considerable importance in teaching, I need do no more than give one simple example of its application, and that only to direct pencils of such small divergence that spherical aberration may be neglected. A perfectly obvious set of modifications is introduced when we treat of oblique pencils, and pencils of large divergence, but students capable of understanding these do not require the adoption of such elementary methods of explanation.

Take, then, the case of light refracted at a concave spherical surface, bounding a substance denser than air. If the incident and refracted rays make (small) angles α and β with the axis of the surface, and if γ be the angle between the normal at the point of

incidence and the axis, these angles being the respective divergences, we have ***rigorously*** by the law of refraction

$$\sin(\gamma - a) = \mu \sin(\gamma - \beta),$$

or, *approximately*,

$$\gamma - a = \mu(\gamma - \beta),$$

or

$$\mu\beta - a = (\mu - 1)\gamma \quad . \quad . \quad . \quad . \quad (1),$$

where μ is the refractive index. [This we may, if we choose, translate into

$$\left(\frac{\mu}{v} - \frac{1}{u}\right)y = \frac{\mu - 1}{r}y,$$

where y is the distance of the point of incidence from the axis, and the rest of the notation is as usual. In this form we see that, to our approximation, the result is independent of y.]

In (1) we have $\gamma = 0$ for a plane surface, and $\mu = -1$ when there is reflection instead of refraction.

Hence for a reflecting surface the meaning of (1) is—"the sum of the divergences of the incident and reflected rays is twice that of the normals to the surface." If the incident rays be parallel, the reflected rays diverge twice as much as do the normals.

At the second surface of a thin lens (1) becomes

$$\frac{1}{\mu}\beta' - \beta = \left(\frac{1}{\mu} - 1\right)\gamma',$$

which, compounded with (1), gives

$$\beta' - a = (\mu - 1)(\gamma - \gamma'),$$

which may be thus translated—"A lens produces *a definite change of divergence* on any direct pencil—and the change is $\mu - 1$ times the difference of the divergences of the normals to its surfaces."

Hence that a divergence may be changed into an equal *negative* divergence, it must be equal to half the change produced by the lens; *i.e.*, when the object and image are equidistant from the lens, their common distance from it is double the focal length of the lens.

4. On the Structure of the *Palæozoic Crinoids*. By Professor Wyville Thomson.

(Abstract.)

The best known living representatives of the Echinoderm Class Crinoidea are the genera *Antedon* and *Pentacrinus*—the former the feather stars, tolerably common in all seas; the latter the stalked sea lilies, whose only ascertained habitat, until lately, was the deeper portion of the sea of the Antilles, whence they were rarely recovered by being accidentally entangled on fishing lines. Within the last few years Mr Robert Damon, the well-known dealer in natural history objects in Weymouth, has procured a considerable number of specimens of the two West-indian *Pentacrini*, and Dr Carpenter and the author had an opportunity of making very detailed observations both on the hard and the soft parts. These observations will shortly be published.

The Genera *Antedon* and *Pentacrinus* resemble one another in all essential particulars of internal structure. The great distinction between them is, that while *Antedon* swims freely in the water, and anchors itself at will by means of a set of "dorsal cirri," *Pentacrinus* is attached to a jointed stem, which is either permanently fixed to some foreign body, or, as in the case of a fine species procured off the coast of Portugal during the cruise of the Porcupine in the summer of 1870, loosely rooted by a whorl of terminal cirri in soft mud. Setting aside the stalk, in *Antedon* and *Pentacrinus* the body consists of a rounded central disc and ten or more pinnated arms. A ciliated groove runs along the "oral" or "ventral" surface of the pinnules and arms, and these tributary brachial grooves gradually coalescing, terminate in five radial grooves, which end in an oral opening, usually subcentral, sometimes very excentric. The œsophagus, stomach, and intestine coil round a central axis, formed of dense connective tissue, apparently continuous with the stroma of the ovary, and of involutions of the perivisceral membrane; and the intestine ends in an anal tube, which opens excentrically in one of the interradial spaces, and usually projects considerably above the surface of the disc. The contents of the stomach are found uniformly to consist of a pulp

composed of particles of organic matter, the shields of diatoms, and the shells of minute foraminifera. The mode of nutrition may be readily observed in *Antedon*, which will live for months in a tank. The animal rests attached by its dorsal cirri, with its arms expanded like the petals of a full-blown flower. A current of sea-water, bearing organic particles, is carried by the cilia along the brachial grooves into the mouth, the water is exhausted in the alimentary canal of its assimilable matter, and is finally ejected at the anal orifice. The length and direction of the anal tube prevents the exhausted water and the fœcal matter from returning at once into the ciliated passages.

In the probably extinct family Cyathocrinidæ, and notably in the genus *Cyathocrinus*, which I take as the type of the Palæozoic group, the so-called CRINOIDEA TESSELLATA, the arrangement, up to a certain point, is much the same. There is a widely-expanded crown of branching arms, deeply grooved, which doubtless performed the same functions as the grooved arms of *Pentacrinus;* but the grooves stop short at the edge of the disc, and there is no central opening, the only visible apertures being a tube, sometimes of extreme length, rising from the surface of the disc in one of the interradial spaces, which is usually greatly enlarged for its accommodation by the intercalation of additional perisomatic plates, and a small tunnel-like opening through the perisom of the edge of the disc opposite the base of each of the arms, in continuation of the groove of the arm. The functions of these openings, and the mode of nutrition of the crinoid having this structure, has been the subject of much controversy.

The author had lately had an opportunity of examining some very remarkable specimens of *Cyathocrinus arthriticus*, procured by Mr Charles Ketley from the upper Silurians of Wenlock, and a number of wonderfully perfect examples of species of the genera *Actinocrinus*, *Platycrinus*, and others, for which he was indebted to the liberality of Mr Charles Wachsmuth of Burlington, Ohio, and Mr Sidney Lyon of Jeffersonville, Indiana; and he had also had the advantage of studying photographs of plates, showing the internal structure of fossil crinoids, about to be published by Messrs Meek and Worthen, State Geologists for Illinois. A careful examination of all these, taken in connection with the description

by Professor Lovén, of *Hyponome Sarsii*, a recent crinoid lately procured from Torres Strait, had led him to the following general conclusions.

In accordance with the views of Dr Schultze, Dr Lütken, and Messrs Meek and Worthen, he regarded the proboscis of the tesselated crinoids as the anal tube, corresponding in every respect with the anal tube in *Antedon* and *Pentacrinus*, and he maintained the opinion which he formerly published (Edin. New Phil. Jour., Jany. 1861), that the valvular "pyramid" of the Cystideans is also the anus. The true mouth in the tesselated crinoids is an internal opening vaulted over by the plates of the perisom, and situated in the axis of the radial system more or less in advance of the anal tube, in the position assigned by Mr Billings to his "ambulacral opening." Five, ten, or more openings round the edge of the disc lead into channels continuous with the grooves on the ventral surface of the arms, either covered over like the mouth by perisomatic plates, the inner surface of which they more or less impress, and supported beneath by chains of ossicles; or, in rare cases (*Amphoracrinus*), tunnelled in the substance of the greatly thickened walls of the vault. These internal passages, usually reduced in number to five by uniting with one another, pass into the internal mouth, into which they doubtless lead the current from the ciliated brachial grooves.

The connection of different species of *Platyceras* with various crinoids, over whose anal openings they fix themselves, moulding the edges of their shells to the form of shell of the crinoid, is a case of "commensalism," in which the mollusc takes advantage for nutrition and respiration of the current passing through the alimentary canal of the echinoderm. *Hyponome Sarsii* appears, from Professor Lovén's description, to be a true crinoid, closely allied to *Antedon*, and does not seem in any way to resemble the Cystideans. It has, however, precisely the same arrangement as to its internal radial vessels and mouth which we find in the older crinoids. It bears the same structural relation to *Antedon* which *Extracrinus* bears to *Pentacrinus*.

Some examples of different tesselated crinoids from the Burlington limestone, most of them procured by Mr Wachsmuth, and described by Messrs Meek and Worthen, show a very remarkable

convoluted plate, somewhat in form like the shell of a *Scaphander*, placed vertically in the centre of the cup, in the position occupied by the fibrous axis or columella in *Pentacrinus* and *Antedon*. Mr Billings, the distinguished palæontologist to the Survey of Canada, in a very valuable paper on the structure of the Crinoidea, Cystidea, and Blastoidea (Silliman's Journal, January 1870), advocates the view that the plate is connected with the apparatus of respiration, and that it is homologous with the pectinated rhombs of Cystideans, the tube apparatus of Pentremites, and the sand-canal of Asterids. Messrs Meek and Worthen and Dr Lütken, on the other hand, regard it as associated in some way with the alimentary canal and the function of nutrition.

The author strongly supported the latter opinion. The perivisceral membrane in *Antedon* and *Pentacrinus* already alluded to, which lines the whole calyx, and whose involutions, supporting the coils of the alimentary canal, contribute to the formation of the central columella, is crowded with miliary grains and small plates of carbonate of lime; and a very slight modification would convert the whole into a delicate fenestrated calcareous plate. Some of the specimens in Mr Wachsmuth's collection show the open reticulated tissue of the central coil continuous over the whole of the interior of the calyx, and rising on the walls of the vault, thus following almost exactly the course of the perivisceral membrane in the recent forms. In all likelihood, therefore, the internal calcareous network in the crinoids, whether rising into a convoluted plate or lining the cavity of the crinoid head, is simply a calcified condition of the perivisceral sac.

The author was inclined to agree with Mr Rofe and Mr Billings in attributing the functions of respiration to the pectinated rhombs of the Cystideans and the tube apparatus of the Blastoids. He did not see, however, that any equivalent arrangement was either necessary or probable in the crinoids with expanded arms, in which the provisions for respiration, in the form of tubular tentacles and respiratory films and lobes over the whole extent of the arms and pinnules, are so elaborate and complete.

5. On the Formation and Decomposition of some Chlorinated Acids. By J. Y. Buchanan.

1. *On the Rate of the Action of a Large Excess of Water on Monochloracetic Acid at* 100° C.—When monochloracetic acid is heated with water, double decomposition takes place, glycollic and hydrochloric acids being formed; and conversely, when glycollic acid is heated with hydrochloric acid, it is converted into monochloracetic acid and water. A similar reaction takes place with the two monochloropropionic and corresponding lactic acids, and probably with all their homologues.

The task which I have set myself is to study these reactions, in so far as they are dependent upon temperature, duration of reaction, and relative mass of reacting substances. In the present communication, I give the results of experimenting upon monochloracetic acid with a very large, practically infinite, excess of water at 100° C.

The monochloracetic acid was purchased from Dr Marquart, of Bonn, and rectified. What passed between 180° and 190° was used for the following experiments:—A watery solution of it was made which contained in a litre 32·4 grms., and showed a specific gravity = 1·0124, whence the chloracetic acid and the water were mixed in the proportion of one molecule of the former to 164 molecules of the latter.

As the increase of the acidity of the solution is the measure of the decomposition which takes place, it is easily determined by titration. For this purpose a solution of caustic soda was generally employed, although in the earliest experiments baryta water was made use of.* The saturating power of these reagents was

* Berthelot (Ann. de Chim. et de Phys. [3], LXV., 401) made use only of baryta, his objections to potash and soda being that they always contain carbonate, and that their salts with organic acids always have a more or less alkaline reaction. The first of these objections may be got rid of by keeping the solution, freed from CO_2 in the first instance by lime water, in a number of *small* bottles filled full up to their tightly fitting corks. The second I have found not to apply to the bodies here in question. There is no doubt, however, that baryta solution does present considerable advantages in the greater ease with which it can be procured in a state of absolute purity; and that any carbonic acid which it may absorb is at once eliminated, thereby, how-

ascertained by means of a very carefully prepared normal sulphuric acid, containing 49 grms. H_2SO_4 in a litre. 10 CC. of this acid saturated 42·7 CC. caustic soda, and 41·8 CC. baryta water, whence one litre caustic soda contains 9·3677 grms. NaHO, and one litre baryta water 20·450 grms. BaH_2O_2. 10 CC. of the above-mentioned chloracetic acid saturated 14·7 CC. caustic soda and 14·4 CC. baryta water.

In every experiment 10 CC. chloracetic acid solution were sealed up in a tube, and introduced directly into the boiling water bath. After the reaction was finished, it was transferred immediately to a vessel of cold water. By this means the time of heating up to 100° and of cooling down again to the surrounding temperature was reduced to a minimum.

The chloracetic acid solution was prepared in the middle of last November, and although it has now stood at the ordinary temperature of the laboratory for over four months, its saturating power has not changed to a sensible extent. It is true, however, that it gives a slight opalescence with solution of nitrate of silver. It appears then that the decomposition of monochloracetic acid by a large excess of water at the ordinary temperature is infinitely slow.

In the experiments at 100° C. the same quantity, namely, 10 CC. of the acid solution, was invariably employed. In the following table showing the results, the first column contains the duration of the experiment in hours; the second the number of CC. caustic soda or baryta water required to saturate the resulting acid, and the third gives the percentage chloracetic acid decomposed as calculated from column 2. No fraction smaller than 0·5 is given, this being the limit of possible errors of observation:—

ever, altering the strength of the solution. My principal objection to it was its great tendency to crystallise even in solutions a long way removed from saturation.

TABLE

TABLE I.—$C_2H_3ClO_2 + 164H_2O$ at 100° C.

Duration of Experiment in Hours.	Number of CC. required for neutralisation.		Percentage of $C_2H_3ClO_2$ Decomposed.
	Soda.	Baryta.	
0	14·70	14·40	0·0
2	15·55	...	6·0
4	16·35	...	11·0
6	16·85	...	14·5
11	18·10	...	23·0
14	18·80	...	28·0
16	19·30	...	31·5
18	19·85	...	35·0
21	20·30	...	38·0
24	20·95	...	42·5
27	21·35	...	45·0
30	22·15	...	51·5
33	22·55	...	53·5
37	22·95	...	56·0
43	23·90	...	62·5
48	24·45	...	66·0
72	...	25·40	76·5
96	...	26·20	82·0
120	27·57	...	87·5
144	28·00	...	90·5
192	28·40	...	93.0
332	28 95	...	97 0
430	29·05	...	97·5

The following Gentlemen were elected Fellows of the Society:—

JAMES GEIKIE, Esq.

THOMAS E. THORPE, Ph. D., Lecturer on Chemistry in the Andersonian Institution, Glasgow.

Monday, 17*th April* 1871.

The Hon. LORD NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. Notes on the Antechamber of the Great Pyramid. Based on the Measures contained in vol. ii. "Life and Work at the Great Pyramid," by C. Piazzi Smyth. By Captain Tracey, R.A. Communicated by St John Vincent Day, Esq., C.E., F.R.S.E.

In considering the authority for the division of the sacred cubit into 25 inches, we have, first, the architectural fact that the Queen's chamber, containing the visible expression of that cubit, stands in or upon the 25th course of masonry, comprising the whole Pyramid. And here, though not strictly bearing on the case, may be mentioned a connection between the lengths of the two passages (the first ascending, and the horizontal passages) leading to that chamber, remarkable when expressed in inches, of which 25 make a cubit.

Thus, the length of the first ascending passage from the axis of descending passage to north wall of Grand Gallery (see p. 54, v. ii., L. and W.)* = 1544·4 B. I., or 1542·9 inches, of which 25 make a sacred or Pyramid cubit, and which for the future we will term "Pyramid inches."

Now, this length of 1542·9 P. I.—25 = 1517·9 P. I.—is the exact length of the horizontal passage from north wall of the Grand Gallery to the north wall of the Queen's Chamber—

E.g., length of horizontal gallery (see p. 57, v. ii., L. and W., last line), } = 1519·4 B. I.

1·5

1517·9 P. I.

* In this paper the following abbreviations are used: "L. and W.," for "Life and Work at the Great Pyramid;" B. I. = "British Inches;" P. I. = "Pyramid Inches." Pyramid Inch = British Inch $\times$ 1·001.

But on entering the Antechamber, we find this particular measure or $\frac{\text{sacred cubit}}{25}$ (which we have termed the Pyramid inch, to avoid expressing that particular measure of length by the algebraical x) not only typified, but expressed, and most notably in the granite leaf, whose precise functions have never yet been explained.

For there—on a stone immediately in front of an unmistakable symbol of division into five—we find a raised boss, with a single straight edge exactly $\frac{1}{5}$ of a Pyramid or sacred cubit in length, and consequently representing 5 of these inches.

The thickness of this boss along the whole line of 5 inches is exactly $\frac{1}{5}$ of that line, $\frac{1}{25}$ of the same cubit, or precisely the inch we are in search of.

Further, the centre of this boss is exactly one inch from the middle of the Antechamber, its distance from either side being 19·5 and 21·5 inches from the west and east walls respectively, and, consequently, it is one inch to the west of centre (just as the niche in the Queen's Chamber, marking the whole 25 inch cubit by the breadth of its flat top, is also 25 inches removed from the central vertical line of the wall in which it is formed).

It may be argued that all these expressions of an inch in the Antechamber depend upon the shape and position of a stone that was not necessarily placed there by the architect of the Pyramid.

Let us, therefore, seek some connection with the grander features of the building, both for the stone itself and the particular measure of length, of which we are thus far led to consider it the standard.

The following calculation shows that a line drawn from the angle of the great step at an angle of 26° 18′, or parallel to the true axis of the Grand Gallery, passes about 1·13 inch below the centre of the bottom of the upper stone forming the granite leaf, or the one that bears the boss.

Vol. ii. L. & W. pp. 93, 96.	B. I.
North end of step to north side of leaf (omit boss)	= 134·3
„ south „	= 150·55
	2)284·85
Distance of centre of leaf from north end of step,	142·42

Height of bottom of leaf above floor, .	.	43·7
,, lower stone of leaf, .	. .	27·75
,, junction of the stones above the floor,	=	71·45
Now, 142·4 × ·494, or nat. tan. of Grand Gallery angle,	=	70·32
		1·13 B.I.

∴ A line ‖ to axis of Grand Gallery, drawn from ∠ of Great Step, passes 1·13 B. I. below centre of joint of leaf.

P. 96 L. & W. This and the next calculation.

Distance of south wall of Antechamber from ∠ of Step = 229·6 B.I.
229·6 × ·494 (nat. tan. Grand Gallery ∠) = 113·42 ,,

show that the same line produced, strikes the south wall of the Antechamber at a height of 113·42 B. I. from the floor. As the boss is to the west of the centre of the room, we turn to that side, and find that the height of the granite wainscot there, where it bears against the south wall, is 111·8 inches or 1·62 B. I. lower than the spot indicated. But, on examining the course of the axis* itself of the Grand Gallery when produced, the following calculation shows that it passes through the lower stone of the leaf at a distance of 0·8 inch below its centre on its northern side, and on being produced strikes the south wall of the Antechamber at a height above the floor of 104·02 B. I., or just an inch above the height of the wainscot on the east side, which reaches an altitude of 103·1 B. I.

Thus connecting the inch, the granite leaf, and the rest of the building in a manner that none but the original Designer could have introduced.

P. 96 L. & W.

North side of leaf (omit boss) from north side of step = 134·3 B. I.

Height of bottom of leaf above floor, . . .	43·7	(P. 99 L. & W.)
One-half height of lower stone,	13·9	,,
Height of centre of lower stone,	57·6	
But 134·3 × ·494	= 66·24	
and axis of ascending passage continued through Grand Gallery is 9·4 B. I. below ∠ of Step†	= 9·4	
	= 56·8	= Height at which axis of Grand Gallery strikes lower stone on north side, or (57·6 − 56·8) or 0·8 B. I. below centre of stone.

* That is, axis of 1st ascending passage continued through Grand Gallery.

† See next calculation.

P. 74 L. & W.

Vertical height of Great Step— East, 35·8 B. I. West, 36·2 — 36· mean.	B. I. Height of ‖ axis = 113·42 − 9·4 = 104·02 = Height of true axis of Grand Gallery above the floor.

Vertical height of northern entrance to Grand Gallery (p. 70 L.&W.) is $53{\cdot}2 - \frac{53{\cdot}2}{2} = 26{\cdot}6$ = height of axis which subtracted from 36· =

9·4 = vertical height of $\angle$ of Great Step above the point where the axis of first ascending passage passes into it.

But the axis of the Grand Gallery, the most important line in the whole building, having so signally pointed out the importance of the lower stone of the leaf, let us examine it also in terms of the inches we are led to connect so closely with it. Taking the mean of all the measures given, the calculation following shows that the cubical contents of that part of the stone not sunk in the grooves

$$= 15{\cdot}7 \times 41 \times 27{\cdot}7 = 17830{\cdot}5 \text{ British inches.}$$
$$\underline{\ 17{\cdot}8}$$
$$= 17812{\cdot}7 \text{ Pyramid inches.}$$

P. 99 L. & W.

Thickness—East end of leaf,	. .	15·4
„ West „	. .	16·
	Mean, .	15·7 B. I.
Height,	. . . . .	27·5
„	. . . . .	28·
	Mean, .	27·7 B. I.

P. 100 L. & W.

Width, 41 B. I.—this measure being taken on the leaf itself, and on the same side as the boss.

Log. 15·7 = 1·1958997
„ 27·7 = 1·4424798
„ 41· = 1·6127839

= 4·2511634 = log. of 17830·5 British inches.

The Ark, or Laver by theory, and the Pyramid Coffer in practice, contain 71321·25 B. I. = 71,250 P. I., the quarter of which, or 17812·5 Pyramid inches (the volume of this particular stone), is the Chomer or Homer of sacred standard.

The remarkable result thus obtained induces a further examination of the position of this stone.

We remark that the base of this stone (lower stone) is in the same horizontal plane as three other well defined lines of the antechamber—viz., the division between the courses of the wainscot on the east wall, and the tops of the doors in the north and south walls.

It is to be noticed that the refined workmanship of the granite wainscoting has been most fully developed to the south of the leaf.

We will thus examine that portion first. The granite leaf itself and the granite walls mark off above the horizontal plane a certain space.

The dimensions of this part of the plane are—

In length varying from	(1.) 79·0 B.I.	to 79·1 B.I.
In breadth	(2.) 41·2	to 41·45 B.I.
While at the height of	(3.) 27·5	to 28 B.I. there

runs across it the joint line of the leaf.

(1.) P. 96 L. & W.—North end of step to south side of leaf, E. 150·3
W. 150·8

Mean 150·55

North end of step to south end of antechamber, E. 229·4
Do. do. W. 229·8

Mean 229·6

Length, East side, 229·4
150·3

79·1

Do. West, 229·8
150·8

79·0

Mean 79·05

(2.) P. 93 L. & W.—41·45

41·2

82·65

41·325 Mean.

(3.) P. 99 L. & W.—27·5

28·

55·5

27·75 Mean.

The already acquired facts give us good reason to look upon the 25th part of the sacred cubit as an unit of measure that may be safely used in at least the antechamber of the great Pyramid, and we only argue in conformity with other teaching of the Pyramid in assuming that the volume of the lower stone of the leaf *may* also be an *unit* of *volume* for antechamber cubical measures.

Thus if we take the lowest readings, a cubical space of 27·5 × 41·2 × 79·0 B.I., or (1.) 89507·0 B.I. is marked out; or (2.) 5·019 of our volume unit.

$$
\begin{array}{llrl}
 & & \text{B.I.} & \\
(1.) & \text{Log. of} & 27{\cdot}5 & = 1{\cdot}4393327 \\
 & \ldots & 41{\cdot}2 & = 1{\cdot}6148972 \\
 & \ldots & 79{\cdot}0 & = 1{\cdot}8976271 \\
 & & \text{B.I.} & \\
 & \ldots & 89507{\cdot}0 & = 4{\cdot}9518570
\end{array}
$$

and (2.) $\dfrac{89507{\cdot}0}{17830{\cdot}5} = 5{\cdot}019$

Practically 5 volumes of the lower stone of the leaf, and therefore $\frac{1}{40}$th of the lower course of the king's chamber.

For that has been shown (by Professor Piazzi Smyth) equal to 2000 baths, or 50 coffers, therefore the space in the antechamber

Equals . . .	50 baths
or	5 chomers
of which last our unit represents .	1

We have consequently the Hebrew chomer standing, as it were, at the end of a measure of 5 times its own capacity, as in the

king's chamber has been found the coffer in one 50 times its own content. The rest of the granite-lined chamber, of which the above formed part, may also be worthy of consideration. Its length and breadth are the same as that of the portion already considered, while its height is determined by that of the containing wainscots. But these, as we have already seen, are determined by the heights at which the south wall is touched, the one by the axis of the (first ascending passage produced through the) Grand Gallery prolonged into the antechamber, and the other by a line parallel thereto drawn from the angle of the great step. But as it would be evidently giving either undue weight to use it alone, let us take (as the following calculation shows) the average height of the two —viz., (1.) 108·72 B.I.

Taking the highest readings of the dimensions, we obtain—(2.), 108·72 × 79·1 × 41·45 B.I., or 356460·4 B.I. (3.), we find therein 19·99, &c. of the units we have seen reason to employ, or so close on 20 as to justify our acknowledging *intention* in the size.

(1.)—H. of ‖ axis, . .	113·47
„ grand gallery axis produced	104·07
	2)217·54
	108·72 mean.

(2.) Log. of	108·72	=	2·0363094
	79·1	=	1·8981765
	41·45	=	1·6165245
	356460·4	=	5·5520104
Minus log.	17830·5	=	4·2511634
(3.)	19·99, &c.	=	1·3008470

Granting that, we have another noteworthy connection established between the antechamber and king's chamber, as there the volume of the lower course has been shown (by Professor Smyth) to equal 50 coffers, or 200 of our units, while here we have its tenth part, or 20 units equalling 5 coffers.

It will doubtless be objected that in one instance we have used the highest, and the other the lowest readings of the measures. Just proportion teaches that the product of the means should be of no less value than that of the extremes.

Let us then take the means of those two sets of numbers, whose extremes only we have been using heretofore, and employ them in

connection with other dimensions of that marked horizontal plane already alluded to.

Examination of it shows that it is broadly divided into two portions, by the leaf resting on it; and the linear measures of the two rectangles thus formed are respectively, the northern one—

(1.)		(2.)
41·7	P. 96, V. 2, L. & W.	P. 99, V. 2, L. & W.
41·45	P. 93, „	21·0
41·2	„ „	
41·45 mean.		

$$\{(41{\cdot}45 \times 2) + (21{\cdot} \times 2)\} = 82{\cdot}9 + 42 = 124{\cdot}9$$

and the southern one—

(3.) See (1) page 426.* (4.) See (2) page 427.*

$$\{(79{\cdot}05 \times 2) + (41{\cdot}3 \times 2)\} = (158{\cdot}1 + 82{\cdot}6) = 240{\cdot}7$$

British inches, 365·6
·36

or in Pyramid inches, 365·24

roughly divided into ⅓ and ⅔rds of No. of days in a year.

The perimeter of the chamber at the ceiling (363 inches) had pointed out the probability of our finding some of the external proportions of the pyramid repeated here; and as there we find the "year" in terms of 4 cubits, or 100 inches, so here we have a "year" of inches; and as there the grander and external year is intimately connected with the height of the pyramid through π, so here we find, through the same medium, a connection with the length of the chamber, a mean of three measures of which gives 116·32 for its length in pyramid inches, for taking 365·24 as circumference, diameter = 116·26.

P. 95 L. & W.—Length of antechamber, 116·3
... ·8
... ·2

Mean 116·43 British inches.
·11

116·32 Pyramid inches.

$$\begin{aligned} \text{Log. of } 365{\cdot}24 &= 2{\cdot}5625783 \\ \pi &= {\cdot}4971499 \\ 116{\cdot}26 &= 2{\cdot}0654284 \end{aligned}$$

* These numbers refer to pages of this volume.

Or an approximation to π, as represented by a "year" of inches marvellously close both in the numbers representing the circumference and diameter, and reproducing here the grander proportions of the external form of the pyramid.

It is to be remembered that the "year" of inches was divided roughly into $\frac{1}{3}$ and $\frac{2}{3}$ds, and the three stones of the ceiling and the three cuts on the wainscot seem to point to some important division by 3.

We have seen π playing so important a part in deciding the height of the pyramid and the length of the Antechamber, that we may at any rate try what a division by 3 will do.

On the base of the pyramid the "year" which represents circumference (or, as regards the height of the pyramid π) was expressed in units of 100 inches. Have we any chance of finding not circumference, for we already have our "year" of inches, but diameter, or radius, as a purely mathematical expression as regards π, when expressed in say the same terms of 100 inches?

Taking π as represented by 314·159, &c. Pyramid inches, we find diameter + radius expressed very closely, as $\frac{2}{3}$ and $\frac{1}{3}$ of the height of the antechamber (*i.e.*, 149·2″).

But when we divide π itself (still expressed in terms of R = 100 Pyramid inches) by 3, we obtain the figures 104·72, which strike us as being an approximation to the height of the wainscot on the east wall (103·1); but when we refer to the grand gallery axis (to whose connection with the east wainscot our attention has already been drawn) we find a still closer approximation (viz., 104·06 P.I.) to the expression of $\frac{\pi}{3}$.

But $\frac{\pi}{3}$ is a curious expression, and not much used in calculations I am conversant with, except in one instance; but that instance bears on the case, as it is in the calculation of volume of spheres, cones, and also pyramids, the area of whose base is expressed in terms of π.

It may be advantageous to note here the connection between the volumes of pyramids and spheres. The content of a pyramid is mathematically expressed thus.

$$V = \frac{a.h.}{3},$$

where a = area of base,
and h = height of pyramid.

But in the purely mathematical form of pyramid we are led to consider

$$a = \pi R^2$$

$$h = R\left(=\frac{D}{2}=\frac{1}{2}\right), \text{ when V would equal } \frac{\pi R^3}{3} : \text{ but in a sphere,}$$

$$\text{volume} = 4\,\frac{\pi R^3}{3}.$$

So that in the case of the great hemispherical molten sea, whose content = 50 lavers, a pyramid of the same base and height would contain 25 lavers, 100 homers, or five of the largest marked-off space in the antechamber whose content has already been pointed out.

This may certainly lead us to infer, that as up to the antechamber our measures have been lineal and superficial; now, on the other hand, we must be prepared for cubical measures with, perhaps, also some concerning the content of spheres, cones, or pyramids.

Commencing our investigation at the horizontal marked plane previously referred to, we remember in its most highly finished portion that its smallest dimensions are 79·0 B. I. and 41·2 B. I., and here we may notice that their sum $\left(\begin{array}{r} 79{\cdot}0 \text{ B.I.} \\ 41{\cdot}2 \quad\; \\ \hline 120{\cdot}2 \text{ B.I.} \end{array}\right)$, 120·2 B.I. or 120·1 P.I. is very close upon the radius of the hemisphere that the presence of $\frac{\pi}{3}$ has led us to refer to. The precise figures standing thus:—

Radius of $\frac{1}{2}$ sphere whose volume = 3,562,500 P.I. (= lower course of King's Chamber = "Molten Sea") is 119·371 P.I.

When vol ume of sphere = 3562500 × 2 cubic inches.

Required its radius:

Now V. of sphere $= \frac{4\pi R^3}{3}$

$\therefore 7125000 = \frac{4\pi R^3}{3}$

$\therefore R^3 = \dfrac{7125000}{\frac{4\pi}{3}}$

$\therefore R = \sqrt[3]{\dfrac{7125000}{4 \cdot 1887902}}$ log. $7 \cdot 125 \cdot 000 = 6 \cdot 8527849$

log. $\frac{4}{3}\pi = 0 \cdot 6220886$

$= 119 \cdot 371$ $3)\overline{6 \cdot 2306963}$

$119 \cdot 371 = 2 \cdot 0768987$

But we are getting on too fast. Now in spite of the presence of π are we to suppose the circle squared practically, as we have imagined, when suggesting that the area of the base of a square pyramid might be represented by πR^2?

To seek an answer to that question we must go back to that part of our investigation, where we had reason to believe that the connection between 116·3 and 365·24 was intentionally introduced as an exponent of the relation between diameter and circumference, and we may not unreasonably test the accuracy of our deductions by finding the area of the circle there expressed, trusting that if we are working in the right direction this step may lead to some further proof of its being so.

But in so doing we should use the figures only as a guide to the *intentions* of the Great Architect, and having as we believe learnt that the "year" of inches symbolises a circle of 365·256, &c., we may take as our starting-point the more accurate diameter represented by $\frac{356 \cdot 256}{\pi}$ or 116·264 pyramid inches.

To proceed.

The area of a circle whose diameter is 116·264 is 10,616·65.

This number in itself does not seem peculiarly suggestive, but when we recollect how remarkably both the east wainscot and granite floor* point to an accurately marked square of 103 Pyramid

* Viz. the east wainscot, a vertical line 103 inches high, and of the floor, a special portion constructed in granite showing a horizontal line 103 inches long.

inches whose area = 10,609, we think we have advanced in the right direction and shown that the builder here places for our instruction and guidance another practical illustration of the importance and use of π, its former application being lineal, and this superficial. And here we stay to point out how these curious proportions, coincidences, and symbols become legible when read by the units of length and volume supplied by the architect of the pyramid himself, and extant (let us hope) to this day in the very spot where their use first becomes imperative.

For though the proportions remain the same whether expressed in inches, feet, or metres, they only become vocal as it were when read by the units there prepared and hung up near them.

What should be the next step in the process of inductive argument?

The sides and perimeter of this square (of 103·0 P.I.) are so obviously connected with the length and breadth of the King's Chamber, as exactly $\frac{1}{4}$, and $\frac{1}{2}$ thereof, that a consideration of the area of *its* floor would perhaps be the next step, guided too by the admonition we fancy we have received on passing through the antechamber, that cubical and not simply linear or superficial measures should occupy us in the chamber ultimately attained.

With what results this has been done over the area of *that* floor, we already know, from Taylor, Smyth, Petrie, and Day, results too so overwhelmingly important, that though the tables of the Law, written by the hand of the Omniscient, have been lost to man, we have here inscribed by the great architect of the pyramid the very essence of all legislation, so exact and so scientific in all its branches, as far as we can penetrate, that it is indeed "ennobling to the mind of man to contemplate."

2. Experiments and Observations on Binocular Vision. By Edward Sang, Esq.

(*Abstract.*)

This communication was chiefly directed to the question whether the idea of distance be obtained from the adjustment of the eyes to distinct vision, or from the convergence of their axes. The case of the chameleon was cited as one in point, since that lizard

directs its eyes each to a separate object, but habitually, when about to strike its prey, brings both eyes to bear upon it. Several experiments, mostly suggested by Wheatsone's inquiries, were cited, and the conclusion was arrived at, that, although the adjustment for direct vision concur in the formation of the estimate of distance, the convergence of the eyes plays the principal part.

3. On the Fall of Rain at Carlisle and the neighbourhood. By Thomas Barnes, M.D.

In this communication, the author offers remarks on journals kept by Dr Carlyle, in the city of Carlisle, from 1757 to 1783 inclusive; by the Rev. Joseph Golding, at Aikbank, near Wigton, Cumberland, from 1792 to 1810 inclusive; and by himself at Bunkers Hill, two and a half miles west of Carlisle, which is situate 184 feet above the sea-level. The author gave tables showing the quantity of rain of each month and year included in these periods. From the averages, it appears that about twice as much rain falls in each of the latter months of the table as in the month of April; and about one-third less rain falls in the first six months of the year than in the last six months, and that April is the driest month of the year.

4. Mathematical Notes. By Professor Tait.

1. On a Quaternion Integration.

A problem proposed to me lately by my friend T. Stevenson, C.E., for constructing what he calls a *Differential Mirror*, when attacked directly led to the equation

$$S . d\rho\left(\frac{\beta + \alpha V\alpha\rho}{\rho}\right)^{\frac{1}{2}}\rho = 0 ,$$

where α is a *unit*-vector, perpendicular to β.

By another mode of solution it was easy to see that the integral must be of the form

$$T\rho - T(\beta + \alpha V\alpha\rho) = \text{constant}.$$

It may be instructive to consider this question somewhat closely, as the form of the unintegrated expression is certainly (to say the least) at first sight unpromising.

The problem was: to construct a reflecting surface from which rays, emitted from a point, shall after reflection diverge uniformly, but *horizontally*. Using the ordinary property of a reflecting surface, we easily obtain the first written equation. By Hamilton's grand "Theory of Systems of Rays," we at once write down the second.

The connection between them is easily shown thus. Let ϖ and τ be any two vectors whose tensors are equal, then

$$\left(\frac{\tau+\varpi}{\tau}\right)^2 = 1 + 2\varpi\tau^{-1} + (\varpi\tau^{-1})^2$$
$$= 2\varpi\tau^{-1}(1 + S\varpi\tau^{-1}),$$

whence, to a scalar factor *près*, we have

$$\left(\frac{\varpi}{\tau}\right)^{\frac{1}{2}} = \frac{\tau+\varpi}{\tau}.$$

Hence, putting $\varpi = U(\beta + \alpha V\alpha\rho)$ and $\tau = U\rho$, we have from the first equation above

$$S.d\rho\,[U\rho + U(\beta + \alpha V\alpha\rho)] = 0.$$

But

$$d(\beta + \alpha V\alpha\rho) = \alpha V\alpha d\rho = -d\rho - \alpha S\alpha d\rho,$$

and

$$S.\alpha(\beta + \alpha V\alpha\rho) = 0,$$

so that we have finally

$$S.d\rho U\rho - S.d(\beta + \alpha V\alpha\rho)U(\beta + \alpha V\alpha\rho) = 0,$$

which is the differential of the second equation above. A curious particular case is a parabolic cylinder, as may be easily seen geometrically. The general surface has a parabolic section in the plane of α, β; and a hyperbolic section in the plane of β, $\alpha\beta$.

It is easy to see that this is but a single case of a large class of integrable scalar functions, whose general type is

$$S.d\rho\left(\frac{\sigma-\rho}{\rho}\right)^{\frac{1}{2}}\rho = 0,$$

the equation of the reflecting surface; while

$$S(\sigma - \rho)d\sigma = 0$$

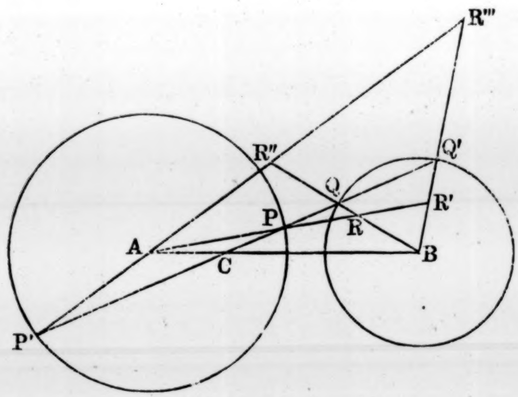
is the equation of the surface of the reflected wave: the integral

of the former equation being, by the help of the latter, at once obtained in the form

$$T\rho + T(\sigma - \rho) = \text{constant}.$$

2. On the Ovals of Descartes.

The following results were obtained lately while I was considering how most simply to describe by working sections surfaces analogous to that treated in the preceding note. They are so elementary that it is not likely that they can be new, but as they are novel to myself, and to several mathematicians whom I have consulted, I bring them before the Society :—



Let two coplanar circles be described, with centres A and B. Take any point, C, in the line of centres, and draw a line CPQ, cutting the circles in P and Q. Find the locus of R, the intersection of AP and BQ.

Expressing that CPQ is a straight line, we have, if θ and ϕ be the angles at A and B respectively,

$$\frac{\overline{AP}\sin\theta}{\overline{AP}\cos\theta \pm \overline{AC}} = \pm \frac{\overline{BQ}\sin\phi}{\overline{BC} \pm \overline{BQ}\cos\phi},$$

or

$$\overline{AP}\cdot\overline{BC}\sin\theta \pm \overline{AC}\cdot\overline{BQ}\sin\phi = \pm\overline{AP}\cdot\overline{BQ}\sin(\theta+\phi),$$

which, by substituting the sides of ARB for the sines of the angles opposite them, becomes

$$\overline{AP}\cdot\overline{BC}\cdot\overline{BR} \pm \overline{AC}\cdot\overline{BQ}\cdot\overline{AR} = \pm\overline{AP}\cdot\overline{BQ}\cdot\overline{AB} \quad \ldots\ldots \quad (1)$$

which is the general equation of Cartesian Ovals.

When $\overline{AP} \cdot \overline{BC} = \overline{AC} \cdot \overline{BQ}$ the curve becomes an ellipse or hyperbola. Of this the simplest case is

$$\overline{AP} = \overline{BQ}\,,\ \overline{BC} = \overline{CA}\,.$$

The normal at R is in all cases parallel to

$$\overline{AP}\,.\,\overline{BC} \cdot U(BR) \pm \overline{AC}\,.\,\overline{BQ} \cdot U(AR)\,,$$

because we have

$$d\,.\,AR = d\,.\,BR\,.$$

But the general equation (1), on account of the identity

$$\overline{AP} \cdot \overline{BC}\,.\,\overline{BQ} \pm \overline{AC}\,.\,\overline{BQ}\,.\,\overline{AP} = \pm \overline{AP} \cdot \overline{BQ} \cdot \overline{AB}\,,$$

may be written more simply, as

$$\overline{AP} \cdot \overline{BC}\,.\,\overline{RQ} - \overline{AC}\,.\,\overline{BQ} \cdot \overline{PR} = 0\,, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

a very singular and suggestive form; holding true, as it does, for all four points, R, R′, R″, R‴, in the figure.

Hence the normal is

$$\frac{U(BR)}{\overline{RQ}} \pm \frac{U(AR)}{\overline{PR}}\,,$$

which may be constructed by drawing at R a tangent to the circle circumscribing the triangle PQR. When the curve is a conic this line is parallel to CPQ, because by the condition above we have in this case

$$\overline{RQ} = \overline{PR}\,.$$

Of course the mode of tracing here adopted is at once capable of being effected mechanically.

The results above are easily derived from the general equation of Cartesian Ovals

$$er \pm e'r' = a\,,$$

by writing it in the form

$$e(r_0 + e'x) \pm e'(r_0{}' \mp ex) = a\,,$$

and showing from this that QP cuts AB in a fixed point.

But by a purely quaternion process it is easy to give in a very simple form the equation of the locus of R *when* C *is not in the line* AB. Let CA, CB, CR be denoted by α, β, ρ respectively, and let

$\overline{\mathrm{AP}} = a$, $\mathrm{BQ} = b$. Then, by expressing that CP and CQ coincide in direction, we have at once the equation

$$\mathrm{V} . [\alpha + a\mathrm{U}(\rho - \alpha)] [\beta + b\mathrm{U}(\rho - \beta)] = 0 ,$$

in which the above results are included as a very particular case, and whose geometrical interpretation is elegant. It is a mere Scalar equation, since $\mathrm{V}\alpha\beta$ is a factor of the left side, and may be omitted.

Added, May 4th, 1871.—I have just been informed by Professor Cayley that the above results, so far as they concern the Cartesian Ovals, are to be found (some actually, some virtually) in Chasles' *Aperçu Historique*, a work of which, to my great regret, I have never been able even to see a copy.

The following Gentleman was elected a Fellow of the Society:—

John Smith, M.D., F.R.C.S.E.

Monday, 1*st May* 1871.

Dr CHRISTISON, President, in the Chair.

The following Communications were read:—

1. On the remarkable Annelida of the Channel Islands, &c. By W. C. M'Intosh, M.D.

The extraordinary richness of the littoral region and the deeper water surrounding Guernsey and Herm, as well as the marked southern character of many of the Annelidan types, formed, for instance, an excellent comparison with the ample series of specimens which the dredgings of Mr Jeffreys in the Shetland seas had lately brought before us; or, again, with the valuable collections procured during the expeditions of the "Porcupine," in 1869 and 1870, the former chiefly from the Atlantic, the latter from the same region and the Mediterranean.

The object of the present paper is to give a short notice, chiefly

of the structural, or other, peculiarities, of the remarkable Nemerteans and Annelids found in this expedition, and of certain interesting questions in zoology connected therewith.

Amongst the Nemerteans is the curious *Ommatoplea spectabilis* of De Quatrefages, a species of much interest, in so far as its discoverer stated that it was furnished with a peculiar horny pectinated structure in its proboscis. Careful examination showed that the latter has a strictly Ommatoplean anatomy, the longitudinal bands of the reticulated layer of the pinkish organ being very apparent. In *Prosorhochmus claparedii,* Keferstein, the granules of the external circlet of glands round the stylet-region of the proboscis are unusually large and distinct. The granular basal sac of the central stylet is of a peculiar shape, having a straight border and sharp angles posteriorly, and obtuse angles at the sides anteriorly. The pale setting of this apparatus is comparatively limited in bulk; and the curved fibres of the region behind the latter pass outwards and forwards in a very distinct manner. The development of the ova in the bodies of the females of this viviparous species is very similar to that of the free ova and their products in other Ommatopleans, space being formed for the growing embryos by the enormous dilatation of the ovisacs. Indeed, the larger young specimens, which are often doubled within the body of the parent, appear to be in cavities produced by the coalescing of many ovisacs; at any rate, it is clear that to describe them, as former authors have done, as simply within the body-cavity of the worm, is wanting in structural accuracy. It seems to be a further stage of the type of development observed in *Nemertes carcinophilus,* Kolliker (*Polia involuta,* Van Beneden), in which, after the deposition of the majority, a few are left in the body of the parent for subsequent evolution. A still more remarkable Nemertean is the *Borlasia elisabethæ,* M'I., from Herm, a large species with a pointed, eyeless snout. In this form the powerful muscular layers of the body-wall are tinted of a fine reddish hue, so that the resemblance in this respect to the muscles of the higher animals is striking. The proboscis is extremely slender in proportion to the bulk of the animal, and its muscular walls are comparatively thin. A reddish coloration was frequently observed in the living animal at the white belts, showing that some contained fluid tinted the

cutaneous tissues during its passage. On puncturing the swollen anterior end, a copious exudation of a reddish-brown fluid occurred. This presented many fusiform and clavate corpuscles, probably from the proboscidian fluid; but there were also a vast number of minute granules, of a yellowish colour by transmitted light, though reddish in mass, which doubtless belonged to the blood-proper. Many of the latter bodies showed a contraction in the middle, so as to resemble the outline of a figure of eight.

In regard to the Annelids Proper, it is found that the northern *Aphrodita aculeata* and *Lœtmonice filicornis*, Kbg., are replaced by the southern *Hermione hystrix*, which occurs in great abundance in water from 10 to 20 fathoms in depth. Amongst the *Polynoidæ*, *P. areolata*, Grube, is remarkable in having greatly swollen cirri. The dorsal bristles are not very robust, while the ventral are in two sets, if the ends alone are viewed, but form a regularly diminishing series from the dorsal to the ventral surface as regards length of tip. The scales are boldly areolated. In this species there is a series of well-marked circular muscular fibres towards the outer half of the vertical coat of the proboscis. The new *Harmothoë marphysæ* accompanies *Marphysa sanguinea* in its tube.

The remarkable forms of the *Phyllodocidæ* and *Hesionidæ;* the great abundance of the *Nereidæ*, and the uses of the latter as bait, were next detailed.

The representatives of the Eunicidæ are very plentiful. Besides the gigantic *Marphysa sanguinea*, there occur *Marphysa belli*, *Eunice harassii* or *norvegica*, and *Eunice gallica*. The allied forms *Lysidice ninetta* and *Blainvillea filum* are also abundant, and impart a character to the fauna of the region. The same may be said of *Prionognathus Kefersteini* and *Staurocephalus rubrovittatus*.

Chœtopterus norvegicus and other phosphorescent Annelida were then examined, and the facts observed in these, as well as in other luminous invertebrates were shown to give no support to the Abyssal Theory of Light as expounded in the "Report (1869) of H. M. ship 'Porcupine.'"

The structure and habits of the Annelida frequenting muddy ground in the Channel Islands, and the examination of those and other marine invertebrates elsewhere, exhibited grave objections to another theory, lately brought forward by Dr Carpenter ("Porcu-

pine" Report for 1870), viz., that the barrenness of the deeper parts of the Mediterranean is due to the turbidity (from mud) of the bottom-water.

2. Note. On the Use of the Scholastic Terms *Vetus Logica* and *Nova Logica*, with a Remark upon the corresponding Terms *Antiqui* and *Moderni*. By Thomas M. Lindsay, M.A., Examiner in Philosophy to the University of Edinburgh.

During the earlier part of the middle ages, or until the middle of the eleventh century, students of logic had a very incomplete knowledge of the logical works of Aristotle. They knew the translations which Boethius had made of Porphyry's Εἰσαγωγή, of Aristotle's περὶ κατεγορίων, and of his περὶ ἑρμηνείας, and they knew little else. Their labours did not go beyond the reproduction of, and commenting on, these old Greek writings.

Towards the beginning of the twelfth century, however, the gradual diffusion of knowledge had brought with it acquaintance with the remaining treatises of Aristotle's Organon. The old translations of Boethius were recovered, and new translations were made. We are told that " Jacobus Clericus of Venetia translated from Greek into Latin certain books of Aristotle, and commented on them, namely, the Topica, the Analytics Prior and Posterior, and the Elenchi, although," adds the chronicler, "an earlier translation of these same books may be had."* This was in 1128 A.D. It is more than probable that Roscellinus, who flourished 1080–1100, knew more of Aristotle's writings than the treatises on the Categories and on Interpretation. Abelard (b. 1079—d. 1142) must have known the greater part of Aristotle's Organon, and John of Salisbury (who died 1180), we know, knew the whole of it.

Hence, whereas at the middle of the eleventh century the knowledge of Aristotle was confined to acquaintance with the two first

* " Jacobus Clericus de Venetia transtulit de græco in latinum quosdam libros Aristotelis et commentatus est, scilicet Topica, Anal. priores et posteriores et Elenchos, quamvis antiquior translatis super eosdem libros haberetur." Robert de Monte Chronica ad Ann. 1128, in Pertz, Monument. viii. 489. Quoted from Prantl, Geschichte der Logik ii. p. 99.

books of the Organon, along with the Introduction of Porphyry, at the middle of the twelfth century there were two distinct sources of knowledge of Aristotle's opinions on Logic—that derived from the "old" tradition from the books on the Categories, and on Interpretation, and from the Introduction of Porphyry, and that derived from a "new" tradition from recovered translations made by Boethius of the Prior and Posterior Analytics, of the Topics and of the book on Fallacies, and from new translations.

This new tradition was looked upon with considerable mistrust by several of the steady going old schoolmen. It disturbed their view of logic. They had constructed a very fair well-rounded system from the material supplied by the old tradition. It had been sufficient for them then, and they wanted nothing new now. Even supposing that these new treatises were Aristotle's, they would not admit them to be logical, or, if they went so far, they would not allow them to have any real importance. The old doctrine had done very well for them and their fathers before them, and it might serve every one else. They saw no need for any change. On the other hand, more enterprising students were vastly taken with these new treatises, and found that they contained Aristotle's real logic. They revealed to them the doctrine of the syllogism, and its application in demonstrative, probable, and fallacious material of knowledge. The new tradition was Logic, the old not more than an introduction, even if worthy of that place.

When we consider that logic, with all its verbal niceties, was more studied than anything else in these days, we find in the very fact of these two different traditions, and the two ways of accepting them, all the elements for a severe and widely extended quarrel: and the quarrel soon arose. On the one side, the zeal shown in studying and commenting upon these new treatises was wholly attributed to the love of novelty, and the new opinions concerning logic and its sphere, which were coming into fashion, were set down as due to a restless, shallow, modern spirit. The logic of the new tradition was called the "*Nova Logica*," and those who advocated it, "*Moderni*." On the other hand, the Moderni thought that their opponents were prejudiced against their opinions, simply because they were not the old ones, and they despised them as old world thinkers, who had not the breadth of view required to accept

anything, however good in itself, which differed from their old theories. They called the logic of the old tradition the "*Vetus Logica*," and its upholders "*Antiqui*."

Now, curiously enough these terms had been applied half a century before, and in a very different manner. When Roscellinus had startled the orthodox world by saying that *universals* were only "*flatus vocis*," and had drawn many heretical conclusions in logic and in theology, from this doctrine, his opponents said that he was the author of a "new" kind of logic, and called his followers "moderni." The "old" logic, of the days of Roscellinus, treated logic from a *realist* point of view, the "new" logic treated logic from a nominalist point of view (so far as the words "realist" and "nominalist" can be used with accuracy of any doctrine at this early period of scholasticism). The Antiqui of the time of Roscellinus became realists in the time of Thomas of Aquino, and the "moderni" were the nominalists of later days.

Here then we have a confusion in the terminology, on the one hand Vetus Logica meant the introduction of Porphyry, the treatises on the Categories, and on Interpretation; Nova Logica, the Prior and Posterior Analytics, the Topics and the book on Fallacies; Antiqui, those who thought that Logic Proper was contained in this Vetus Logica; Moderni, those who thought that this Nova Logica was the true Logic. On the other hand, Vetus Logica meant logic treated from a realist point of view; Nova Logica, logic treated from a nominalist point of view; while Antiqui and Moderni corresponded very much to the latter terms of Realist and Nominalist.

This confusion does not really last throughout the period of Scholasticism. The meaning of the terms did fluctuate somewhat, as all terms do, but upon the whole they preserved a great uniformity of meaning. "Vetus" and "Nova Logica," became dissociated from "Antiqui" and "Moderni," with which they were at first so closely united, and, curiously enough, while the one set of terms kept to one of their primitive meanings, the other set kept to the opposite meaning. "Vetus" and "Nova Logica" were used of divisions of Aristotle's Organon; while Antiqui and Moderni became more or less, though never quite, equivalent to Realist and Nominalist.

"Vetus Logica," from the middle of the twelfth down to the beginning of the sixteenth century, meant the logic taught in the *εἰσαγωγή* of Porphyry, and in the *περὶ κατηγοριῶν* and the *περὶ ἑρμηνείας* of Aristotle.

"Nova Logica," during the same period, meant the logic of Aristotle's *ἀναλυτικὰ πρότερα, ἀναλυτικὰ ὕστερα, τοπικά* and *περὶ σοφιστικῶν ἐλέγχων.* This is the almost invariable scholastic use of the terms. Any other is accidental and variable.

Now, this assertion is made against the greatest authority in the history of scholastic Logic, Professor Prantl of Munich, whose "Geschichte der Logik im Abendlande," is one of the most trustworthy and laborious efforts in historical research. Dr Prantl recognises, as every one must do, that the meaning given here to "vetus" and "nova logica" was one of the principal scholastic uses of the terms, and every quotation to be made from logical treatises in support of our view of the question appears in his notes, but he seems to think that the expressions retained their relation to the names "Antiqui" and "Moderni," and that any signification which belongs to them apart from these names is entirely subordinate. He connects the term "Nova Logica" with the partly grammatical, partly logical additions to the doctrine which first became popular through the Summulæ Logicales of Petrus Hispanus; * he makes it occupy the middle place between the "old" logic and the "Ars Magna" of Raymond Sully; and he has proved by a quotation from a dialogue in that curious and amusing Manuale Scholarium or Mediæval Students' Guide-book, given in Zarnacke's Deutschen Universitäten im Mittelälter, that when the Antiqui were hard pressed by the Moderni, they always retired on the "Vetus Logica" as their stronghold. †

* *Prantl* believes that this addition to logic is due to a Byzantian influence, and therefore believes that the Summulae of Petrus Hispanus is almost a Latin translation from the Greek of Psellus. Sir W. Hamilton and many other authorities refuse to admit this Byzantian influence, and hold that the Greek work of Psellus is a copy or translation from the Latin of Petrus Hispanus. *Prantl, Gesch. der Logibr.*, ii. p. 264. *Hamilton Discus.* 2nd ed., p. 275.

† C. iv. De altricatione viarum et disciplinarum.

Camillus. Hunc magistrum tu quasi ad cœlum attuliste tamen modernus est.

Bartoldus. Quid tum?

It is not to be supposed that two names, especially when embodied in such vague words as "old" and "new" should have preserved the same invariable meanings in every writer during a period of three centuries. We may, therefore, admit, without prejudice to our statement, that the terms "Vetus" and "Nova Logica" did bear those significations which Prautl gives to them, and did preserve a more or less continuous connection with the terms "Antiqui" and "Moderni." But it may be proved that, from about the middle of the twelfth century down to the middle of the fifteenth at least, the first meaning which the term *Vetus Logica* would suggest to a mediæval student was "the logic treated in the Predicables of Porphyry, and in the Categories and De Interpretatione of Aristotle;" while the first meaning suggested by the term *Nova Logica*, was "the logic treated in Aristotle's Prior and Posterior Analytics, his Topics, and his book on Fallacies."

This may be directly proved from the quotations which Prantl himself gives.

Lambert of Auxerre, who lived in the middle of the 13th century, says, "Logica traditur in omnibus libris logicæ, qui sunt sex, sc. liber prædicamentorum, liber Peryermenias, qui nunc dicuntur *vetus logica*, liber Priorum, Posteriorum, Thopicorum et Elenchorum, *qui quatuor dicuntur nova logica.*"—Cf. Prantl, iii. p. 26.

Cam. Nihil ab eo deinceps audiam.

Bart. Eo stultior es, si doctrinam despicis. Nam non solum realistæ verum etiam moderni magnam partem philosophiæ consecuti sunt.

Cam. Sed versantur in sophismatibus tantum, veram doctrinam aspernantur.

Bart. Offendis veritatem, nam erudissimi viri reperiuntur inter modernos. Nonne audisti, in quibusdam terris eos possidere integras universitates? ut Viennæ Erfordiæ, utque quondam hic erat. Nonne arbitraris, doctos hic bonosque fuisse? Et nostro aevo adhuc reperiuntur?

Cam. Scio quidem et intelligo, sed fama eorum parva est. Elaborant solum *in parvis logicalibus* et sophismaticis opinionibus.

Bart. Non recte intelligis, nam clari sunt in enunciationibus et syllogismis. Non reperies artium studiosos, qui syllogismos ceterasque species argumentationis facilius noscant quam moderni.

Cam. Et in vera scientia nihil sciunt.

Bart. Quam mihi facis veram scienciam?

Cam. *Predicabilia Porphyrii, cathegorias Aristotelis*, in quibus aut parum noveant aut nihil.—p. 11, 12.

Duns Scotus, who died in 1308, calls Syllogistic, *i.e.*, the Prior and Posterior Analytics and the Topics, the "Nova Logica," and the Categories, with the De Interpretatione, the "Vetus Logica."

In the 14th century we have commentaries Super Veterem Artem, *e.g.*, by Antonius Andreas, by Walter Burleigh, and by Gratiadei of Ascoli (Esculanus, as he is commonly called), and these are invariably expositions of the Predicables of Porphyry, the Categories, and the De Interpretatione of Aristotle.

Esculanus (d. 1341) says plainly, "Ars autem nova, quæ tota versatur circa ratiocinationem, oportet quod distinguatur secundum diversam considerationem eius; potest autem ratiocinatio dupliciter considerari, uno quidem modo simpliciter sine applicatione ad materiam aliquam, et alio modo considerari potest cum applicatione ad materiam specialem. De ratiocinatio quidem sumpta in sua comitate, agitur in libro priorum, sed ratiocinatio sumpta cum applicatione ad materiam specialem distinguitur; quia aut applicatur ad materiam demonstrativam; ac sic agitur de ipsa, in libro posteriorum; aut etiam applicatur ad materiam dialecticam. In materia autem dialecticam potest fieri ratiocinatio recta et ratiocinatio sophistica. De ratiocinatione recta agitur in libro topicorum; et de ratiocinatione sophistica in libro elenchorum." *

There is, however, another source of evidence which Prautl has not in this reference carefully investigated—the regulations and decrees of the universities. When any term whatever is found in a university decree, we may take it for granted that its signification there was the standard one for the time being, and when we find the same terms occurring in the regulations of almost all the principal universities with the same meaning, we are warranted in adopting that meaning as the real signification of the term.

These terms, "Vetus" and "Nova Logica," are frequently found in the regulations of the mediæval universities, and they invariably mean the logic taught in the first two, and the logic taught in the last four, of the treatises of the Organon.

* Commentaria Graciadei Esculani ordinis predicatorum. In totam Artem veterem Aristotelis, f. 1.

Thus as early as 1215 * the students of Paris University are commanded to read the *books of Aristotle* on Logic,—both the "Vetus" and the "Nova Logica."

In 1309 we find, among the Statuta Collegii Cluniacensis, a statute concerning scholars studying philosophy, in which students are told to work at—first the Summulæ in the college; then the Vetus Logica; and lastly the Nova Logica, either in the college or outside.† This passage is important, because it shows that the Summulæ are not part of the Nova Logica; elsewhere Summulists are distinguished from Logicos.

In 1366, at the reformation of the Faculty of Arts, it is ordained that students attending lectures in this faculty read the whole of the *vetus ars*, four books of the Topics and the books of the Elenchi, the Prior or the Posterior Analytics completely, and the books De Anima in whole or in part.‡

In the munimenta of the University of Oxford, published by the Master of the Rolls, we have many references to the vetus and nova logica; and in all cases the reference is evidently to books of Aristotle's Organon.§

Thus Artistæ are told, in 1340, that, before they can "incept" in arts, they must first have sworn that they have read two logical books at least, one of the vetus logica, and the other of the nova.||

In the munimenta of the University of Glasgow, of the date 1460, or thereabout, we find it enacted in the regulations about reading in logic—"Ordinaria vero audienda sunt hæc; primus sc. in Veteri Arte liber universalium Porphyrii, liber Predicamentorum Aristotelis, duo libri Peri Hermeneias ejusdem. In Nova Logica duo libri priorum, duo posteriorum, quatuor ad minus Topicorum, sc. primus, secundus, sextus, et octavus, et duo elenchorum. . . . Item audiantur libri extraordinarii . . . in logica textus Petrus

* Bulæus. Hist. Univ. Paris, iii. p. 82.

† Ibid., iv. p. 122.

‡ Item quod audierunt veterem Artem totam, librum Topicorum, quoad 4 libros, et libros Elenchorum, Priorum aut Posteriorum complete; etiam librum de Anima in tota vel in parte.—*Bul. Hist. Univ. Paris*, iv. 390.

§ Munimenta Acad. Oxon. 128, 417, 422. Edited by Anstey.

|| Ibid., 142, cf. 242, 286.

Hispanus cunc syncathegorematibus, tractatus de distributionibus liber sex principiorum." *

This reference is important, because it places those grammatico-logical treatises, which gave a distinctive character to the logic of the moderni, outside of the "nova logica."

In the Liber Decanorum of the University of Prague, the *Vetus ars Aristotelis* is always kept separate from the books of the Prior and Posterior Analytics, the Topics, and the book on Fallacies; † and this division is elsewhere referred to as that of "Vetus" and "Nova Logica." ‡

Aschbach, in his history of the University of Vienna, says that the *Ars Vetus* treated of the Predicables of Porphyry, and of the Categories or Predicaments, and of the de Interpretatione of Aristotle. The *Logica Nova* looked at argumentation as a whole, and considered—(1.) The Resolution or analyses of syllogisms given in the Prior and Posterior Analytics; (2.) Inventive, or ways of discovering true middle terms, given in the Topics; and (3.) Fallacies, given in the libri Elenchorum. Prof. Aschbach shows that Logic, as taught in Vienna, consisted of three parts—the Vetus Logica, which was studied as an introduction; the Parva Logicalia, for the Vienna Students were Moderni; and the Nova Logica.§ The lists which he quotes bears out his statement, with this exception, that after some time the Parva Logicalia, not the "Ars Vetus," came to be looked on as the introduction to Logic.‖

These quotations may, perhaps, serve to prove our assertion, that the scholastic use of the terms "vetus" and "nova logica" is almost exclusively confined to the designation of parts of the

* Munimenta Univ. Glasg., ii. 25, 26. This reference I owe to Professor Veitch of Glasgow.

† Liber Decanorum Fac. Phil. Univ. Prag. Pars. i. pp. 83, 126.

‡ Ibid., p. 127.

§ Ibid., p. 89, 90.

‖ Ibid., pp. 95, 135, 139, 142, 144, 147, 151, 154, 161. According to these lists a course of lectures on the *Ars Vetus* cost 5 groschen, but, if taken with exercises and colloquia, or quæstiones, it cost 18 groschen. A course on the Parva Logicalia cost 10 groschen, including quæstiones. While a course on the Nova Logica cost 12 groschen, and 36 including quæstiones (p. 95). In the last decade of the 14th century, the course on the Parva Logicalia consisted of 104 lectures, and cost a gulden; the length of the course on the Vetus Logica was the same, and the fee the same; while the courses on the Nova Logica consisted of 132 lectures, and the fee was 35 groschen (p. 352).

Organon of Aristotle—the part earlier and the part later known; and that the meaning of the terms did not vary with the significations of Antiqui and Moderni.

The point discussed in this note is of small importance on its own account, but it is one step, and a rather significant one, in the argument which tends to show that the new life in scholasticism which expressed itself most fully in the 14th century in William of Occam, and which afterwards developed, through the early natural philosophers of Italy, into those scientific methods which have rendered modern science possible, was due to the inborn genius of western Europe, and was not a foreign growth cut from the Greek stock and engrafted on the Latin.

3. On some Abnormal Cones of *Pinus Pinaster*. By Professor Alexander Dickson.

In their celebrated essay, "*Sur la disposition des feuilles curviseriées*,"* the brothers Bravais describe a cone of *Pinus Pinaster* (*Pin maritime*), where the lower part of the cone exhibited secondary spirals 7 S, 12 D (series $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}, \frac{8}{19}$, &c.), while towards the apex the arrangement, in consequence of the disappearance of one of the spirals by 12, changed to 7 S, 11 D (series $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}$, &c.).† They describe another cone of the same species, in which the lower four-fifths exhibited secondary spirals 9 S, 13 D (series $\frac{1}{4}, \frac{2}{9}, \frac{3}{13}, \frac{5}{22}$, &c.), changing at the upper fifth to 8 S, 13 D (ordinary series $\frac{1}{2}, \frac{1}{3}, \frac{2}{5}$, &c.) by suppression of one of the spirals by 9.‡ Such cases, along with some others chiefly in the capitula of *Dipsacus sylvestris*, lead these authors into a discussion of the general question of the possible transition from one arrangement to another by change in the number of secondary spirals. As regards their "curviserial" forms, however, they are disposed only to admit the occurrence of such transitions by way of

* Ann. des Sc. Nat. 2d ser. t. vii.

† *L. c.* p. 93.

‡ *L. c.* p. 103.

convergence of secondary spirals, *i.e.*, by abortion of one, or possibly coalescence of two, resulting in diminution of number. For example, after referring to the possible derivation of an arrangement with 5 and 7 secondary spirals (series $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}$, &c.), from an ordinary one with 5 and 8, by abortion of one of the spirals by 8, they add that "the series 1, 4, 5, 9 . . . does not admit of explanation by the way of abortion, and that one can deduce it from the ordinary series only by supposing a *superfœtation* or addition of a new spiral among the secondary spirals by 8." "This hypothesis," they continue, "appears to us altogether improbable, since in the face of an immense number of instances where two spirals *converge* into one, we cannot on the other hand cite one (apart from rectiserial stems) where one spiral *diverges* into two similar and parallel ones."*

The two cones of *Pinus Pinaster* which form the immediate subject of Dr Dickson's paper, and for which he is indebted to the kindness of R. Smyth, Esq., Emyvale, Co. Monaghan, Ireland, are interesting cases of *convergence* of spirals. These, together with a few other cases already noted by Dr Dickson, seem to throw some additional light upon this question of the origin of variations in the spiral arrangements in a given plant, where not unfrequently spirals belonging to several distinct systems occur.

In the first of the cones received from Mr Smyth, there is at the base a right-handed $\frac{8}{37}$ spiral (series $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}, \frac{8}{37}$, &c.) with the secondary spirals 9 S, 14 D, 23 S. A little above the base, however, two of the 9 spirals to the left run into one, leaving, from that point up to about the middle of the cone, an arrangement of secondary spirals 8 S, 14 D, 22 S = a left-handed bijugate of the series $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}$, &c., with divergence $\frac{5}{18 \times 2}$. About the middle of the cone two of the 14 spirals to the right run into one, leaving, from thence to the top of the cone, an arrangement of secondary spirals 8 S, 13 D, 21 S = a left-handed $\frac{13}{34}$ spiral of the ordinary series $\frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{8}$, &c.

* *L. c.* pp. 104, 105.

The second of Mr Smyth's cones exhibits from the base to near the top a right-handed $\frac{5}{18}$ spiral (series $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}$, &c.) with secondary spirals 7 S, 11 D. Near the top of the cone, however, two adjacent scales of two of the 7 spirals to the left have partially coalesced, and beyond that point the two spirals run into one, leaving an arrangement of secondary spirals 6 S, 10 D = a left-handed bijugate of the ordinary series $\frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{8}$, &c., with divergence $\frac{3}{8 = 2}$.

In the cone of *Pinus Lambertiana*, recently exhibited to the Society, it will be recollected that at the bottom and top of the cone there was a left-handed $\frac{5}{23}$ spiral (series $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}$, &c.); while in the middle was a right-handed bijugate of the series $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}$, &c., where the divergence in each of the two generating spirals $= \frac{5}{12 \times 2}$. In this cone the steepest secondary spirals at the bottom and top were 9 D, 14 S; while those in the middle were 10 D, 14 S.

In connection with the above, Dr Dickson recalled attention to the flower-spikes of *Banksia occidentalis* recently exhibited to the Society, where there were four different arrangements,—viz., one with secondary spirals 7 and 7 = alternate whorls of 7 (or, if preferred, a 7-jugate of the ordinary series with divergence $\frac{1}{2 \times 7}$), giving 14 vertical rows; one with secondary spirals 7 and 6 = a $\frac{2}{13}$ spiral (series $\frac{1}{6}, \frac{1}{7}, \frac{2}{13}$, &c), giving 13 vertical rows; one with secondary spirals 7 and 5 = a $\frac{5}{12}$ spiral (series $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}$, &c.), giving 12 vertical rows; and one with secondary spirals 8 and 5 = a $\frac{5}{13}$ spiral (ordinary series) giving 13 vertical rows.

It will be noted that, contrary to the opinion of MM. Bravais, one arrangement does not necessarily or only originate from another by suppression of parts. To prove this, we have only to

look at the above-mentioned cone of ***Pinus Lambertiana***, where the arrangement in the middle region results from an *augmentation* of parts as compared with the base of the cone; while the spiral at the top, which is the same as that at the base, is, of course, the result of a *diminution* as compared with the middle. It has been already observed by authors, moreover, that in such plants as Cacti and succulent Euphorbias* one vertical row may be split into two, or, conversely, two run into one, thus changing the spiral. Now, as vertical rows are, in one sense, only to be regarded as the steepest secondary spirals (a slight torsion readily converting them into actual spirals), such cases are in all essentials comparable to the above-described cones.

The arrangements above indicated will be rendered very readily intelligible by the accompanying tabular views.†

TABLE A.—*Cone of* Pinus Pinaster (*Mr Smyth—No.* 1).

	S	D	S	D	S	D	S	V	
Top,	1	2	3	5	8	13	21	34 =	$\frac{13}{34}$
Middle,	—	—	2	6	8	14	22	36 =	$\frac{5}{18 \times 2}$
Bottom,	—	1	4	5	9	14	23	37 =	$\frac{8}{37}$

TABLE B.—*Cone of* P. Pinaster (*Mr Smyth—No.* 2).

	D	S	D	S	D	V	
Top,	—	2	4	6	10	16 =	$\frac{3}{8 \times 2}$
Bottom,	1	3	4	7	11	18 =	$\frac{5}{18}$

* The greater number of these plants would be reckoned as truly rectiserial by MM. Bravais. Dr Dickson has no hesitation in referring to such cases in this argument, as he is strongly disposed to doubt as to there being any fundamental distinction between the "rectiserial" and the so-called "curviserial" spirals of these authors.

† In these tables, under S, are indicated the numbers of spirals, generating as well as secondary, running to the *left;* under D, the numbers of those running to the *right;* while under V are indicated the numbers of vertical rows.

TABLE C.—*Cone of* Pinus Lambertiana, *in Museum, Edinburgh Botanical Garden.*

	S	D	S	D	S	V
Top,	1	4	5	9	14	$23 = \frac{5}{23}$
Middle,	—	2	4	10	14	$24 = \frac{5}{12 \times 2}$
Bottom,	1	4	5	9	14	$23 = \frac{5}{23}$

Table D represents the four different arrangements in the flower-spikes of *Banksia occidentalis*, placed in series so as to show how, by slight diminution or augmentation in the number of secondary spirals, one arrangement may be conceived to originate from another. The directions of the spirals to right or left are stated arbitrarily, to suit the purpose of the diagram.

TABLE D.

	D	S	D	S	D	V
No. 1,	—	—	—	7	7	$14 = \frac{1}{2 \times 7}$
No. 2,	—	—	1	6	7	$13 = \frac{2}{13}$
No. 3,	—	1	2	5	7	$12 = \frac{5}{12}$
No. 4,	1	2	3	5	8	$13 = \frac{5}{13}$

It is impossible to reflect on such cases as have been adduced and not be impressed forcibly with the idea that, as regards their production or origination, *diverse spiral arrangements are to be regarded as allied much more according to the numerical correspondence of their secondary spirals and verticals than in proportion to the correspondence of their angular divergences.* Such cases, moreover, show clearly how a generating spiral may change its direction on one and the same axis.

It is perhaps rash to speculate as to how the different systems of spirals in Fir cones originate. On the whole, Dr Dickson is inclined to assume the bijugate of the ordinary system as the fundamental arrangement. He is to some extent confirmed in this view by a remarkable abnormality in a cone of *P. Pinaster*, gathered by him at Muirhouse, near Edinburgh. This cone exhibits a left-

handed $\frac{8}{21}$ spiral. At the base of the cone, however, a number of rudimentary scales of small size and somewhat peculiar shape are intercalated with considerable regularity among the others, so as to appear as projections placed at the intersections of the lines formed by the margins of the larger scales. Now, if these small scales had been disposed with perfect regularity, and had been of equal size with the others, there would have been a left-handed bijugate arrangement, with divergence $\frac{8}{21 \times 2}$. Such a cone, in fact, suggests the possibility of single spirals of the ordinary series being derived from bijugates of the same series by suppression of one half of the scales.

Again, the ordinary trijugates are easily derivable from bijugates, as indicated in Table E.

TABLE E.—*Showing the possible derivation of ordinary Trijugate from the Bijugate Arrangement.*

D	S	D	S	V
—	3	6	9	$15 = \frac{2}{5 \times 3}$
2	4	6	10	$16 = \frac{3}{8 \times 2}$

From the ordinary trijugate, in turn, a spiral of the system, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{2}{9}$, $\frac{3}{14}$, $\frac{5}{23}$, &c., may be simply derived, as indicated in Table F.

TABLE F.—*Showing possible derivation of a Spiral of the System,* $\frac{1}{4}$, $\frac{1}{5}$, *&c., from the Ordinary Trijugate.*

D	S	D	S	D	S	V
1	4	5	9	14	23	$37 = \frac{8}{37}$
—	3	6	9	15	24	$39 = \frac{5}{13 \times 3}$

Again, it is clear that by augmentation of parts, a spiral of the system $\frac{1}{3}$, $\frac{1}{4}$, $\frac{2}{7}$, &c., may be derived from the ordinary bijugate, since the converse (by diminution) actually occurs in the second of Mr Smyth's cones indicated in Table B.

Lastly, the spiral $\frac{5}{22}$, series $\frac{1}{4}, \frac{2}{9}, \frac{3}{13}, \frac{5}{22}$, &c., which Dr Dickson formerly noted as occurring in a cone of *Pinus Pinaster*, in the Museum, Edinburgh Botanic Garden, may readily be derived, as MM. Bravais have suggested,* from a spiral of the series $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}$, &c., thus,

TABLE G.—*Showing possible derivation of a* $\frac{5}{22}$ *Spiral from the System* $\frac{1}{4}, \frac{1}{5}$, *&c.*

D	S	D	S	D	V	
—	1	4	9	13	22	$= \frac{5}{22}$
1	4	5	9	14	23	$= \frac{5}{23}$

The following Gentleman was admitted a Fellow of the Society:—

Rev. Professor CRAWFORD.

Monday, 15*th May* 1871.

PROFESSOR CHRISTISON, President, in the Chair.

At the request of the Council, Professor Tait gave an Address on Spectrum Analysis.

(*The following is a brief Abstract, consisting mainly of the Lecture Notes*):—

I should not have thought of appearing before you to-night to lecture on so hackneyed a subject, had I not been assured by several members of the Council that such an address was really desired by many Fellows of the Society. It is a subject to which I have not paid very special attention, partly because it is in so many and such good hands, and partly because (except from the point of view of theory) it requires for its extension, especially to

* *L. c.* p. 103.

astronomy, very costly instrumental appliances and a great sacrifice of time. And the difficulty of transporting to the Society's rooms from the College the large amount of bulky and delicate apparatus required for its proper illustration, is (as I have just found) so great, that if on any future occasion the Society desire me to give such an address, I shall have to make it a condition that the meeting for that evening be held in my class-room in the University buildings.

The subject of spectrum analysis must always possess great interest for this Society, inasmuch as many of its most distinguished promoters have been, or are, among our Fellows, ordinary as well as honorary, and several of the most remarkable memoirs on various parts of the subject are to be found among our publications.

The objects of spectrum analysis may be briefly enuntiated as follows:—*To make, by optical methods, the qualitative chemical analysis of* (1) *a self-luminous body ;* (2) *an absorbing medium, whether self-luminous or not.*

It is difficult now-a-days, when so many philosophers are engaged almost simultaneously at the same problem, to decide which of their successive steps in advance is that to which should really be attached the title of *discovery* (in its highest sense) as distinguished from mere *improvement* or *generalisation.* You have only to look at the recent voluminous discussions as to the discoverer of the Conservation of Energy, to see that critics may substantially agree as to facts and dates, while differing in the most extraordinary manner as to their deductions from them.* Some of these writers, no doubt, put themselves out of court at once by habitually attributing the gaseous laws of Boyle and Charles to Mariotte and Gay-Lussac. Men who persist in error on a point so absolutely clear as this, show themselves unfit to judge in any case of even a little more difficulty. Others, who strongly support the so-called claims of Mayer in the matter of Conservation of Energy, and who should (to be consistent) therefore far more strongly advocate the real claims of Talbot, Stokes, Ångström, Stewart, &c., to the discovery of spectrum analysis, are found to uphold Kirchhoff as alone en-

* Some frantic partisans of Papin, &c., deny almost all credit to Watt in the matter of the steam-engine! No farther examples need be cited.

titled to any merit in the matter. As a paper by Mr Talbot, on the early history of the subject, is to be read this evening, I shall content myself for the present with the remark, that, of the two objects of spectrum analysis above named, Talbot and Herschel were unquestionably foremost in the enuntiation of the first; Brewster, Ångström, and especially Stokes and Balfour Stewart, in that of the second. Why some of their statements were incomplete or inexact, and what was required to complete or to correct them, will be more usefully stated after I have given some preliminary explanations.

SPECTRUM.—Newton's fundamental experiment.
Reason of separation of colours.
Reason of impurity.
How to obtain a pure spectrum.
Object of trying to do so.
Effect of Additional Prisms.

Note that the source of light in all these experiments has been carbon heated to incandescence by resistance to a powerful current of voltaic electricity.

I. Incandescent solids and liquids give *generally* a continuous spectrum.

Its highest radiation, and the amount of radiation of each wave length, depend on the temperature.

Hence the necessity of using the highest temperature we can obtain.

Illustrate by different lengths of platinum wire heated by current.

II. Gaseous bodies, incandescent, give generally a (limited) number of perfectly definite wave lengths (though under certain circumstances of pressure, &c., they give a continuous spectrum). The number depends for each substance on its temperature and pressure, and their appearance is *characteristic of the substance.* For, under the same physical circumstances, we have always the same effect—as, indeed, must be assumed to be the case, if we think physics can be studied at all. This remark was virtually made by Carnot, and is all that was wanting in Talbot's earliest paper to make it the complete statement of this first part of the subject.

Illustrate by the spectra of the incandescent vapours of
Thallium,
Lithium,
Magnesium,
Sodium.

Illustrate the conductivity of the vapour of the latter by the increased breadth of the spectrum when it is present; also by its effect in improving the spectra of other substances when a weak battery is used.

Hydrogen—by induction-coil.

(Here refer again to Talbot's paper, presently to be read.)

SPECTROSCOPES.—Swan's paper, in Edinburgh Transactions—Introduction of Collimator—estimation of the excessively minute amount of sodium required to give the D line.

UNIVERSAL PREVALENCE OF SODIUM, LITHIUM, &c.

DISCOVERY OF NEW METALS.—Bunsen—Rubidium, Cæsium.
Crookes and Lamy—Thallium.
Reich and Richter—Indium.

DISCOVERIES IN ASTRONOMY AND METEOROLOGY.
Lightning.
Aurora.
Solar prominences and corona.
Nebulæ.
Comets.
Zodiacal light.
Temporary stars.

Huggins, Janssen, Lockyer, Secchi, &c.

III. Absorption by glowing gases, from otherwise continuous spectra.
Fraunhofer's lines (Wollaston).
Reversal of sodium line (exhibit).
Hence *atmospheres* of sun, stars, &c.

Brewster (in Edinburgh Transactions).
Nitric peroxide—effects of heat and pressure.
Atmospheric lines.

Foucault.—Spectrum of incandescent carbon points, seen (by reflection) through the voltaic arc (which itself gives them bright) shows the D lines reversed.

Stokes—about 1850, gave, in consequence of W. H. Miller's very accurate verification that the double bright line of sodium exactly corresponds in refrangibility with the double dark line D, the correct mechanical explanation of the phenomenon, with the mechanical illustrations still very often employed. Given, with general theory of solar and stellar chemistry, ever since (annually) by Thomson in his lectures. Give it.

Ångström—1853.—"Un gaz à l'état d'incandescence émet des rayons lumineux de la même réfrangibilité que ceux qu'il peut absorber."

B. Stewart (Edinburgh Transactions, 1858–9).

Extension of the Theory of Exchanges—The radiating power of a body is equal to its absorbing power, and that for every ray. Based on experimental facts.

Heated pottery ware, with marked pattern, looked at in the dark.

Coloured glasses lose their colour in the fire.

Kirchhoff, Oct. 1859.—Introduction of reasoning more *directly* based on the Second Law of Thermodynamics.

Proof that the absorbing flame must be colder than the source—Exception for Fluorescence.

Kirchhoff and Stewart.—Tourmaline, which polarises common light by absorbing polarised light, gives off, when hot, polarised light like that which it absorbs.

(Note that the discussion of the question of priority on this subject, in papers by Stokes, Thomson, Kirchhoff, and Stewart, in the *Phil. Mag.* 1863, is very interesting, and may still be read with profit).

Fluorescence is Degradation of Energy.

Exhibit Stokes' fundamental Experiments.

The question of priority just alluded to illustrates in a very curious way a singular and lamentable, though in one sense honourable, characteristic of many of the highest class of British scientific men; *i.e.*, their proneness to consider that what appears evident to them *cannot but* be known to others. I do not think that this can be called modesty; it is rather a species of diffidence due to their consciousness that in general their accurate knowledge of the published developments of science is confined mainly to those branches to which they have specially devoted themselves. Their foreign competitors, on the other hand (especially the Germans), are often profoundly aware of all that has been done, or, at least, have some one at hand who is, and can thus, when a new idea occurs to them, at once recognise, or have determined for them, its novelty, and so instantly put it in type and secure it. Neither Stokes nor Thomson, in 1850, seems to have had the least idea that he had hit on anything new, especially as they had a vague recollection that Foucault had previously attacked the problem—the matter appeared so simple and obvious to them—and, but for the fact that Thomson has given it in his public lectures ever since (at first giving it as something well known), they might have thus forfeited all claim to mention in connection with the discovery. I could mention many other striking instances of this peculiarity; one, in fact, appeared in our own *Proceedings* a few months ago; but to consider it more closely would lead me away from the subject of my lecture. It is sufficient to have called attention to a want which could easily be supplied, if we

had anything in this country equivalent to the *Fortschritte der Physik*, but published with considerably less delay.

Detailed study of Solar Spectrum—mainly due to the labours of two men.

Maps by Kirchhoff and Ångström, with the number of elements proved to exist in the sun's atmosphere.

According to Ångström, the following numbers of bright lines given by elements are found exactly coincident with dark lines in the solar spectrum :—

Hydrogen,	4	Manganese,	57
Sodium,	9	Chromium,	18
Barium,	11	Cobalt,	19
Calcium,	75	Nickel,	33
Magnesium,	4 + (3 ?)	Zinc,	2 (?)
Aluminium,	2 (?)	Copper,	7
Iron,	450	Titanium,	118

He notes that Thalén has found 200 coincidences with Titanium lines.

TYPES OF STARS—Secchi.

I. White stars—Scarcely any absorption lines, except those due to Hydrogen, which are strongly marked. Sirius, Vega, &c.

II. Yellow stars—The Sun, Arcturus, Aldebaran, &c.—multitudes of fine lines.

III. Nebulous bands in addition to the fine lines—α Herculis, α Orionis, &c. In Mira Ceti these bands vary with the apparent magnitude. Similar appearances are observed in the spectra of sun-spots. On the contrary, Algol retains the first type through all its periodic changes.

IV. Feeble spectrum crossed by bright lines. The stars of this type are all of small apparent magnitude (*i.e.* of feeble luminosity), and usually of a blood-red colour. Temporary Stars—bright lines of hydrogen.

If to these be added

V. Resolvable Nebulæ—Continuous spectrum, as are those of the nebula in Andromeda, and of many others *not* resolvable ; and

VI. Planetary Nebulæ, and others irresolvable, such as those of Orion, Lyra, &c., where the spectrum consists of a very few bright lines only,

it seems to me that we have a series of indications of what (for want of a better phrase) may be called the *period of life* of a star or group ; beginning with the glowing gases developed by the impacts of the agglomerating cold masses (VI.), * then the almost perfect spectrum of white-hot liquid or compressed gas (V., I.), which (as it becomes colder) suffers absorption by the rise of still colder vapours (II.) ; then, as it farther cools, nebulous bands take the place of sharp lines (III.) ; anon the bursts of glowing gases are

* See the Abstract of my paper on Comets, Proc. R.S.E., 1868–9.

brighter than the photosphere (IV.), and, finally, no light but that of these gases is intense enough to reach us (VI.) That there is energy enough to produce these successive developments is obvious from the fact that, even at their immense distance, the *visible* portions of the nebulæ of Orion and of Argus subtend an angle of nearly *four degrees.*

Application of the spectroscope to determine the RELATIVE VELOCITY OF A STAR, OR OF A GASEOUS CURRENT IN THE SOLAR PHOTOSPHERE, WITH REGARD TO THE EARTH.

Analogy from sound.
Railway whistle.
Tuning-fork experiment.
Similar experiment with organ-pipe.

Finally, ABSORPTION BY BODIES AT ORDINARY TEMPERATURES.

Coloured glasses.
Chlorophyll.
Detection of blood, changes of the blood-spectrum by oxidation, &c., &c.
Microscopic spectroscope.

The following Communications were read :—

1. Note on the Early History of Spectrum Analysis. By H. Fox Talbot, Hon. F.R.S.E.

Newton, in his observations on the spectrum, appears never to have used a narrow aperture. In fact there was nothing, in the existing state of knowledge in his day, to lead him to suppose that this would alter the phenomena.

Wollaston was the first who observed some obscure bands in the spectrum, by viewing with a prism the aperture left by the shutters of his room when nearly closed. It is surprising that this acute philosopher did not follow up the hint thus accidentally presented to him, but contented himself with the rude observation above mentioned.

Fraunhofer was the first who detected the wonderful system of dark lines in the solar spectrum, by viewing a very narrow and accurately formed aperture with an excellent prism, aided by a small telescope. He likewise gave names to the principal dark lines which have been generally adopted, and he measured accurately their refractive indices by mounting the prism on a graduated brass circle movable round a centre.

After completing his observations on the solar spectrum, he

turned his attention to the spectrum of the stars, of which he described several. He likewise described the spectrum of electric light, but only that of sparks passing through atmospheric air. He has likewise left on record a very curious observation on the spectrum presented by the exterior flame of a wax candle. When the bright flame is intercepted by a screen, and only the faint exterior flame viewed, he found it to consist almost entirely of homogeneous yellow light; but his skill as an observer was so great that he perceived this yellow light to consist of two distinct rays very close together, and only separable by an excellent prism, and a very narrow aperture. As he remembered that there was a similar double ray in the yellow part of the solar spectrum which he had named D, the happy thought occurred to him of transmitting solar light through the same aperture. He did so, and found that the two rays of the line D coincided most accurately with the double yellow ray given by the exterior flame of a wax candle. He does not appear to have prosecuted this interesting research further. He merely records the fact. He was not aware that the yellow light of the candle was in any way caused by the presence of *sodium*, the existence of which in a wax candle would probably not occur to any one, unless perhaps to an experienced chemist on the look out for some extraneous substance.

About the same time Sir D. Brewster had been seeking for a source of homogeneous light, for the purpose of improving the microscope by destroying all chromatic aberration of the lenses. See his paper of 1822 in the Transactions of the Royal Society of Edinburgh, vol. ix. p. 433. Although acquainted with the effect of salt on the flame of burning alcohol, he had evidently only cursorily examined it, since he says "salt *or nitre*," which is incorrect, and speaks of its causing the flame to yield "insalubrious vapours." He therefore rejects the use of it, and merely recommends that the alcohol should be "largely diluted with water." The yellow light so obtained he refers to "imperfect combustion" (p. 435), and not in any way to *sodium*, observing that the combustion of paper, linen, cotton, or the flame of a blow-pipe, also contain the same homogeneous yellow light in tolerable abundance. His observations, therefore, have a certain resemblance to those of Fraunhofer.

About the year 1824 or 1825, Dr Wollaston gave one of his

evening parties, to which men of science and amateurs were invited, and it was the custom to exhibit scientific novelties, and to make them the subject of conversation.

On the evening in question I brought as my contribution to the meeting some very thin films of glass (such as are shown in glass-houses to visitors by a workman, who blows a portion of melted glass into a large balloon of extreme tenuity, and afterwards crushes the glass to shivers). Such a film of glass I brought to Dr Wollaston and his friends, and after showing that in the well-lighted apartment it displayed a uniform appearance without any markings, I removed it into another room, in which I had prepared a spirit lamp, the wick of which had been impregnated with common salt. When viewed by this light, the film of glass appeared covered with broad nearly parallel bands, which were almost black, and might be rudely compared to the skin of a zebra. Similar bands, but much fainter, were seen by transmitted light. All present agreed that this curious phenomenon could only be due to the extreme homogeneity of the light of the lamp with the salted wick, which much exceeded any previous estimate of it. It did not occur to any one that evening to procure a lens and a plate of glass, in order to try the effect of the light on Newton's rings. But such an experiment tried soon afterwards revealed an astonishing augmentation of the number of rings visible. I followed up this observation by publishing a paper in 1826 (Brewster's Journal, vol. v. p. 77), in which I determined, among other things, the following facts, namely, that all the salts of soda gave the yellow line D, which I therefore affirmed to be characteristic of sodium. That the salts of potash give a violet light, together with a single red ray situated almost at the end of the spectrum, and with no other light near it. [Subsequently Brewster made careful observations upon this ray, and found it to be coincident with A in the solar spectrum, a remark which recent researches with more powerful instruments have shown to be not entirely exact. Brewster did one great service in pointing out the fact that in inquiries like this an *achromatic* telescope is not necessary.]

The following is a quotation from this paper (vol. v. p. 77):—"The flame of nitre contains a red ray of remarkable nature. This red ray possesses a definite refrangibility, and appears to be cha-

racteristic of the salts of *potash*, as the yellow ray is of the salts of *soda. If this should be admitted, I would further suggest that whenever the prism shows a homogeneous ray of any colour to exist in a flame, this ray indicates the formation or the presence of a definite chemical compound.*"

Further on, speaking of the spectrum of red fire (such as is used in theatres and in fireworks), I said, "the other lines may be attributed to the antimony, strontia, &c., which enter into this composition. For instance, the orange ray may be the effect of the strontia, since Herschel found in the flame of *muriate of strontia* a ray of that colour If this opinion should be correct, and applicable to the other definite rays, *a glance at the prismatic spectrum of a flame may show it to contain substances which it would otherwise require a laborious chemical analysis to detect.*"

An early paper by Herschel has been omitted in its proper place, the year 1822 (Transactions Royal Society of Edinburgh, vol. ix. p. 455). He there shortly describes the spectra of chloride of strontia, chloride of potassa, chloride of copper, nitrate of copper, and boracic acid.

In 1827 (after the publication of my experiments in 1826), he stated in the Encyclopædia Metropolitana, article on Light, p. 438, that salts of soda give a copious and purely homogeneous yellow; those of potash a beautiful pale violet. He also describes the spectra of lime, strontia, lithia, barytes, copper, and iron.

In another paper of mine (Phil. Mag. 1834, vol. iv. p. 114), the flames of strontia and lithia are examined. The following is an extract from this paper:—"The strontia flame exhibits a great number of red rays, well separated from each other by dark intervals, not to mention an orange, and a *very definite bright blue ray.* The lithia exhibits one single red ray. Hence I hesitate not to say that optical analysis can distinguish the minutest portions of these two substances with as much certainty, if not more, than any other known method."

Another passage, taken from the same page, records the first observation of those peculiar rays at the violet end of the spectrum, to which some years later Herschel gave the name of the *lavender* rays. "The flame of Cyanogen separates the violet end of the spectrum into three portions, with broad dark intervals between.

The last of those portions is so widely separated from the rest as to induce a suspicion that it may be more refracted than any rays in the solar spectrum. This separated portion has a pale undecided hue. I should hardly have called it *violet* were it not situated at the violet end of the spectrum. To my eye it had a somewhat whitish or greyish appearance."

This was followed by another paper of mine "On Prismatic Spectra" (Phil. Mag. 1836, vol. ix. p. 3), in which the spectra of gold, copper, zinc, boracic acid, and barytes are described.

Wheatstone, nearly at the same time, published some interesting analogous researches. I regret not to have his paper at hand at present, in order to give a full account of it.

Brewster then took up the subject, and described the spectra produced by the combustion of a great variety of substances, in a paper printed in the Manchester meeting (1842) of the British Association (see Proceedings of the Sections, p. 15). But in the same page there is another short paper by Brewster, of surpassing interest, since he there announces the fact that the bright rays which are characteristic of artificial flames are for the most part those which are deficient in solar light, a fact previously confined to the line D, and discovered, as we have said, by Fraunhofer. These observations of Brewster deserve to be quoted textually. His paper is entitled "On Luminous Lines in certain Flames corresponding to the defective Lines in the Sun's Light."

After noticing Fraunhofer's beautiful discovery as to the phenomena of the line D in the prismatic spectra, Sir David said—"He had received from Fraunhofer a splendid prism, and upon examining by it the spectrum of deflagrating nitre, he was surprised to find the red ray discovered by Mr Talbot, accompanied by several other rays, and that this extreme red ray occupied the exact place of the line A in Fraunhofer's spectrum, and equally surprised to see a luminous line corresponding to the line B of Fraunhofer. In fact, all the black lines of Fraunhofer were depicted in the spectrum in brilliant red light. The lines A and B in the spectrum of deflagrating nitre appeared to be both double lines, and upon examining a solar spectrum under favourable circumstances, he found bands corresponding to these double lines. He had looked with great anxiety to see if there was anything analogous in other

flames, and it would appear that this was a property which belonged to almost every flame."

One thing only was wanting in order to complete this discovery of Brewster's, namely, to explain why the rays which are bright in artificial flames should be dark in the solar spectrum. The explanation of this fact was reserved for later inquirers.

The above is far from exhausting the catalogue of Brewster's researches on the spectrum. He made numerous measurements of Fraunhofer's lines and maps of certain portions of the solar spectrum. He likewise discovered the extraordinary effect of nitrous gas upon the spectrum transmitted through it, which becomes covered with a vast multitude of lines, irregularly disposed, but always appearing in the same places in the spectrum, provided the density and temperature of the gas is the same.

2. On Some Optical Experiments. By H. F. Talbot, Hon. F.R.S.E.

I. On a New Mode of observing certain Spectra.

The attention of the scientific world has been for some years past fully awakened to the importance of observing the spectra exhibited during the combustion of chemical substances. But in making an extensive series of such experiments, it must often happen that the observer has to test substances of which he only possesses a very minute quantity. In that case, before he has viewed the spectrum long enough to feel fully satisfied of its nature, his stock of the substance is exhausted, and he is obliged to leave his observation imperfect. He might perhaps be testing some mineral in his cabinet, of which the native locality was unknown, and he might surmise it to contain a new metal, from its yielding a ray not before seen in the spectrum, yet after a short time his observations on it would come to an end, and he would have no means of showing this ray to other observers. Some years ago the metal thallium was so rare that it was only distributed by a few grains at a time to those who were interested in its discovery; and many of the rarer metals are absent from most chemical laboratories, or only represented by trifling specimens. About four or five years ago I devised a method of remedying, or, at least, greatly diminishing

this inconvenience, which, with some slight recent improvements, I will now proceed to describe. My method was founded on a fact which I had observed many years ago, namely, that the mere presence of a chemical substance in a flame frequently suffices to cause the appearance of its characteristic rays, and that it is not at all necessary that the substance should be consumed and dissipated. This dissipation is an accident, and if by any means it could be prevented, the flame would maintain its characters for a considerable time. For instance, in Brewster's Journal for 1826, vol. v. p. 77, &c., I remarked that alcohol burnt in an open vessel, or in a lamp with a metallic wick, gives but little yellow monochromatic light, while if the wick be of cotton, it gives a considerable quantity, and that for an unlimited time. And I added that I had found other instances of a change of colour in flames, owing to the *mere presence* of a substance which suffers *no diminution in consequence*. Thus, a particle of muriate of lime on the wick of a spirit lamp will produce a quantity of red and green rays for a whole evening without being itself sensibly diminished.

Mindful of these experiments of 1826, when a few years ago I wished to examine the spectra of thallium and other substances, I adopted the following plan :—A grain, or sometimes much less, of the substance was placed in a piece of strong glass tube about one inch long. Short platina wires were inserted into the tube at each end, approaching each other within about half an inch. The ends are then sealed by a blow-pipe, leaving enough of the platina wire outside the tube to allow of its being soldered to a long copper wire. One of these copper wires (with the external portion of the platinum wire soldered to it) was then coated with gutta percha for the space of three or four inches next the tube. To coat the other wire was found unnecessary. The mode of experimenting was as follows. The tube in a horizontal position, having the chemical substance nearly in its centre, was lowered into a glass of water about two or three inches below the surface. The two wires were then connected with a Ruhmkorff's coil, set in action by six of Grove's cells. When the sparks were allowed to pass through the tube, they speedily ignited the substance, and caused it to give forth its characteristic spectrum. Even after the sparks have been passing for several minutes, the tube remains perfectly cold. This

is the object of placing it under water, for if that precaution is not taken the tube will sometimes become very hot, and explode. The gutta percha covering is to prevent the spark passing through the water, and to oblige it to pass through the tube. It is sufficient, as I have said, to cover one wire. If a drop of water has been enclosed in the tube along with the chemical substance, the colours of the spectra are displayed with more vivacity; but if this is done, it is absolutely necessary to have the tube well under water. The bright light given off under these circumstances by strontia, sodium, thallium, and many other substances, is very beautiful, and so permanent that at the close of the experiment the original grain or half grain of the substance does not appear diminished, and even the drop of water is found remaining unchanged. Provided always that the chemical substance is one not liable to decomposition under these circumstances of heat and moisture. In these experiments a small Ruhmkorff's coil was found to answer better than a very large one.

This method might be usefully applied to the illumination of microscopic objects by homogeneous light. If the tube were placed immediately under the stage of the microscope, the full intensity of the yellow light would fall upon the object.

All these experiments were made in the Physical Laboratory of the University of Edinburgh by the kind permission and assistance of Professor Tait.

II. On the Nicol Prism.

Many years ago, when this beautiful and useful optical instrument was new and very little known, I wrote a paper in a scientific journal calling attention to its merits, and recommending its use. It was first described by its inventor in Jameson's Journal for 1828, p. 83. The title of the paper being "*On a Method of so far increasing the Divergency of the two Rays in Calcareous Spar that only one Image may be seen at a time.*" This paper was reviewed in Poggendorff's Annalen for 1833, p. 182, who says—That he perused Mr Nicol's account of his invention with very little hope of its proving successful, but that having constructed the instrument, he found that nothing could answer more perfectly than it did. Having read this testimony to its merits, I had one made by a London optician,

which proved very successful. I then published a paper on it in the Phil. Mag. for 1834, vol. iv. p. 289, from which I must ask leave to make an extract, as a necessary introduction to what I wish to say about it on the present occasion.

My paper begins by quoting the testimony of the German writer to the merits of the instrument, and continues thus:—

"Poggendorff then goes on to say, that as Mr Nicol had not attempted to explain the operation of the instrument, he would endeavour to do so, in which, however, I cannot say that I think he has been entirely successful. Now, it will be observed that the inventor attributed the fact of the instrument's producing only one image to a great 'divergency' which it causes in the images, throwing one of them aside out of the field of view. The German writer follows the same idea, but adds, that in his opinion such divergency is caused by the Canada balsam, whose index of refraction being 1·549, is intermediate between that of the ordinary ray 1 654 and that of the extraordinary ray 1·483, which circumstance will (in his opinion) account for the rays being 'thrown opposite ways.' He adds, that any one 'who was not afraid of the trouble' might easily calculate the path of both rays, a remark which shows that his idea was that they were both transmitted, and diverging from each other. But I find that this great divergency does not, in point of fact, exist, for by inclining the instrument a position may be found in which both images are seen, and they are then very little separated, not more so than they were by the same piece of spar before its bisection and cementation. On gradually altering the position of the instrument, the second image is not seen to move away from the first; but at a certain moment it vanishes suddenly without leaving the smallest trace of its existence behind. Having thus described the appearances as I have found them, I will give an explanation of them, which I hope will be more satisfactory. As long as the rays composing the images are incident upon the Canada balsam at moderate obliquities, it cannot exert any particular discriminating action upon them. But when the obliquity reaches a certain point, one of the images suffers total internal reflexion, because the Canada balsam is (with regard to that image) a less refractive medium than calc spar. But with regard to the other image, it is at the same

moment a more refractive medium than the spar, and therefore it suffers that image to pass alone."

The preceding remarks were published in the year 1834. Soon afterwards I perceived that if my explanation were correct, a Nicol prism might be made, half of calc spar and half of glass. Theory indicated this, but no actual experiment of the kind was made at that time. Recently, however, my attention has been once more directed to this subject, and I have had such an instrument constructed by Mr Bryson, optician, of Edinburgh, with a very satisfactory result. When light has been polarised by an ordinary Nicol prism, it is completely extinguished by the new prism held in a proper position; whereas when two Nicol prisms are combined, a small portion of light generally remains visible.

Either end of the new prism may be held foremost, a result which was not altogether expected. An idea is prevalent that the action of an ordinary Nicol prism is due to the circumstance that one surface of the calc spar is left *rough* to scatter one of the rays. But such is not the case. Both surfaces are highly polished by the best makers, and the ray is not scattered, but reflected, and may be seen by proper management.

3. Note on a New Scotch Acidulous Chalybeate Mineral Water. By James Dewar, F.R.S.E.

It is generally known that this country is extremely deficient in well-marked chalybeate waters. Plenty natural waters, containing small proportions of iron, are to be met with in the United Kingdom; but, with the exception of those of Tunbridge Wells, Harrogate, Sandrock (Isle of Wight), Heartfell, near Moffat, and Vicarsbridge, in the vicinity of Dollar, they contrast very unfavourably with those of the numerous spas of the continent of Europe. If we restrict ourselves to an examination of the chemical characters of the above-mentioned Scotch chalybeates, we observe that the iron is present in large quantities in the form of sulphate, along with sulphate of alumina, on which account they are more nauseous to invalids, and are at the present time rather unpopular.

Recently my brother, Dr Alexander Dewar, Melrose, sent me for

analysis a sample of a new well water, whose peculiarity had previously attracted his attention. A chemical examination of the water in question showed it to be a well-defined acidulous chalybeate, unusually rich in carbonate of iron. The following are the analytical details. (As the surface water gets access at present, a very exhaustive analysis appeared unnecessary):—

Carbonate of iron, . .	17·5	grains per gallon.
Alumina, . . .	1·8	"
Silica, . . .	8·5	"
Sulphate of magnesia, .	7·8	"
Chloride of calcium, .	16·0	"
Carbonate of calcium, .	4·1	"
Alkaline chlorides, .	11·4	"
Total residue, .	67·1	"

Carbonic acid gas per gallon 40 cubic inches.

With the exception of the celebrated "Dr Muspratt's chalybeate," at Harrogate, which contains 10·8 grains per gallon of carbonate of iron, along with 16·0 grains of protochloride, I do not know of any natural water in this country containing such a large proportion of iron in the form of carbonate. And it is to be observed that the water is not associated with a large quantity of other salts.

The well whence the foregoing sample was taken has not been long sunk, and its water is perfectly different from all of those in its immediate vicinity. Should it maintain its present character, I have no doubt that, judging from its own qualities, as well as from its favourable climatic situation, along with the general interest attached to the locality, this chalybeate is certain to recommend itself to the medical profession.

The following Gentleman was admitted a Fellow of the Society:—

Thomas J. Boyd, Esq.

Monday, 29*th May* 1871.

Professor CHRISTISON, President, in the Chair.

The following Communications were read :—

1. On the Homologies of the Vertebral Skeleton in Osseous Fishes and in Man. By Professor Macdonald.

Abstract.

After a brief notice of the seven bi-vertebral segments of the cranium in man:—

1. The hypo-cranial, or the axis and atlas vertebræ, which is adopted as a key to the cranial segments;
2. Para-cranial, or occipital;
3. Wormi-epiotic parietal, or meta-cranial;
4. Sphenoidal, or meso-cranial;
5. Ethmo-frontal—pro-cranial;
6. Nasal, or apo-cranial.
7. Rhino-nasal.

Professor Macdonald gave a short outline of the osteology of the human cranium, in order to trace the homologous osteology of the osseous fishes, or ichthyia.

The great characteristic of the vertebralia is the centro-chord, or axis, extending through the whole length of the animal from stem to stern, around or upon which the vertebral column has been developed. This has been demonstrated in the very earliest type, both by the late Professor Goodsir and Professor Owen in the Amphioxus, where the direction of the anterior portion, as far as the oral cleft, is to the tip of the nose from the anterior portion of the representative of the spinal marrow. The same proof may be adduced from the condition of the early human embryo, where the anterior of the embryo, consisting of the pro-cranium and part of the tubercles of the spine, are at once bent downwards, towards the upturned coccygeal extremity of the spine, where the umbilicus is afterwards formed, when the abdominal or ventral laminæ unite to close in the abdomen. There is another flexure of the pro-cranium and the meso-cranium in warm blooded vertebrata.

It is very important to notice this last flexure as distinctly marking the difference between the warm and cold-blooded animals, and to account for the necessity of the temporal squamo-zygomatic limb-bearing girdle connecting the anterior and posterior cranium.

From this zygoma, or limb-bearing zone or girdle, the maxilla depends as the anterior thoracic limbs, as seen in the annulozoa and arthrozoa. The condyle being articulated in the glenoid cavity, it is the upper or homotype of the brachium and femur, and the homologue of the quadratum of the bird, hypotympanic, and of osseous fishes (28, Owen).

He then directed the attention to the reduced scale of the fish cranium. The general form, from the great depression of the ethmo-frontal segment, prevents the formation of a pros-encephalon, and even the meso-encephalon is crushed back into the III. or wormi-epiotic parietal segment; the only encephalic cavity in the fish cranium, where not only the orbit and the convolutions and olfactory cells, but also the whole otic sensory apparatus with the cerebellum. This segment is closed in by the development of the wormi-epiotic spine, which has hitherto been described by all anatomists, from Cuvier and others on the Continent, and by Professors Owen, Huxley, Parker, and all their followers, as the occipital bone in the fish. A careful re-examination of the subject will correct this general and inconsiderate error. In the osseous fishes the occipital bone still exists in the bi-vertebral condition. It, however, contains the medulla oblongata, and their long spines extend upward, as they do in the human cranium, to nearly the wormi-epiotic spine.

Referring to the archetype of Owen, the basi-sphenoid (5.) was shown to be the last vertebral centrum, from whence the basi-cranium extended, without central joints, to the anterior glabella frontis. (13, incorrectly named vomer) is in fact the premandible or incisor bone. (13.) The vomer is a vertical, or mediastinal double osseous septum, set on the rostrum sphenoides (olivaris) in connection with the perpendicular plate of the ethmoid and septum nasi separating the olfactory cells.

From (4) the wormi-epiotic tuber or spine the upper part of the ischium is attached by a chain of transparent bent scale-bones containing a muscle, seems the principal part of the pelvis; it has a large

tuberosity; from the inner part the ramus rises.* From the inner and lower surface of the tuber ischii the femur (51) descends. It is from the inner articulation in the fishes, instead of the external acetabulum in the human pelvis, that the relation between the tibia (52) and fibula (58) is altered. The fibula is articulated within the head of the tibia; the femur overlaps the upper spine of the head of the tibia. The external malleolus tibiæ is very greatly prolonged, and forms the great osseous sub-opercular cleft, while the internal malleolus fibulæ is embedded in the skin behind the tarsal fin.

The tarsal fin consists of calcaneum (55), astragalus (53), scaphoid (54). These Cuvier named radius and ulna, in which he was followed by Owen, &c. Anterior cuneiform and cuboid tarsals (56). The phalangeal fin rays (57).

The mistaken homology of the pectoral fin for the anterior instead of the posterior extremity baffles all chance of correct homology, and I earnestly hope that all the living homologists will re-examine the subject, and adopt the system which I have wrought out for between forty and fifty years without succeeding to convince the anatomists. I put forth this final appeal of the oldest of living homologists who proposed an original scheme (my friend, Professor Grant, University College, London, introduced that of the brilliant but fanciful Geoffroy St. Hilaire some years earlier), with the firm conviction that ere long, after I have retired, the scheme now proposed will be adopted.

* Owen's Nomenclature.	Macdonald's Nomenclature.
50. Supra-scapula.	50. Ischium.
51. Scapula.	51. Femur.
52. Coracoid.	52. Tibia.
53. Humerus.	53. Astragalus.
54. Ulna.	54. Scaphoid.
55. Radius.	55. Calcaneum.
56. Carpal.	56. Tarsal.
57. Metacarp-phalanges.	57. Tarsal fin rays.
58. Epicoracoid.	58. Fibula.

2. Scheme for the Conservation of Remarkable Boulders in Scotland, and for the indication of their Positions on Maps. By D. Milne Home, Esq.

Among many geological questions which wait solution, there is probably none more interesting or perplexing than the agency by which Boulders or "blocs erratiques," as the French term them, have come to their present sites. I allude, of course, not to blocks lying at the foot of some mountain crag from which they have fallen by the decay or weathering of the overhanging rocks, but to blocks which have manifestly been transported great distances, after being detached from the rocks of which they originally formed part.

That many of the large isolated blocks lying on our mountain sides and on our plains have come from a distance, and by some means of tremendous power, is obvious even to an unscientific observer; and the perception of this truth by the popular mind has, in many cases, so invested these boulders with superstitious interest, that they have received names and given rise to legends, which impute the transport of them to supernatural agents.

There are two circumstances which very plainly indicate that these stones are strangers.

One is, that many of these blocks are on examination found to be different from any of the rocks prevailing in or near the district where they are situated.

The other is, that some of these blocks, whilst excessively hard, —so hard that it is difficult to break off a portion with the hammer, are nevertheless round in form—a form evidently acquired by enormous friction—such friction as would result from being rolled a long way over a rough surface.

The inference drawn from these two facts was confirmed when it was discovered, as in many cases it was, that rocks of the same nature as the block existed in a distant part of the country, and from which, therefore, it had probably come.

These round shaped blocks were the first to attract popular attention. The name given to them in Scotland of *boulders* has no doubt been suggested by their shape.

It is accordingly only the rounded boulders which possess the

traditionary names and curious legends by which many of them are known. Such names as the Carlin's Stane, the Witch's Stane, Pech or Pict's Stone, Clachannadruid, Kirk-Stane, Pedlar's Stane, Thuggart Stane, and Devil's Putting Stane, are all applicable to rounded blocks.

When the geologist turned *his* attention to the subject, it was soon discovered that there were many blocks equally entitled to be called erratic, not round but square shaped; and which, though discovered to belong probably to rocks at a great distance, yet showed signs of little or no attrition. Moreover, many of these angular or sharp-edged blocks were comparatively soft and loose in structure, so that they could not have been rolled, for any considerable distance, without being broken or crushed into pieces, or into sand or mud.

On a more minute inspection and study of these erratic blocks, certain features were noticed which seemed to indicate the forces to which they had been subjected. Thus on many of them, deep scratches, ruts, and groovings were found, as if sharp pebbles or stones harder than themselves had been pushed over them, or squeezed against them under great pressure. It was also observed that, when a block had a long and a short axis, the longer axis was generally parallel with any well marked scratches or striæ on their surface; and moreover that the direction of these striæ frequently coincided with the direction in which the block itself had apparently come from the parent rock.

These circumstances soon led geologists to speculate on the nature of the agencies which could have effected a transport of the blocks. Some blocks are of enormous size, exceeding 1000 tons in weight.* Many, before they could have reached the places where they were found, must have travelled fifty or sixty miles, and have crossed valleys and even ranges of hills. In the county of Berwick, for example, there is a large block of gneiss, a rock which exists nowhere in that county or in the south of Scotland; and if it came from some of the hills in the Highlands, it must have crossed, not only the valley of the Forth, but the Kilsyth, Pentland, and Lammermoor Hills.

* The celebrated block near Neufchätel, called "Pierre à bot," contains about 1480 cubic yards of stone, and is supposed to weigh about 2000 tons.

Sir James Hall and Sir George Mackenzie in this Society, who were the first to study the subject, advocated the idea of diluvial agency. At a later period, ice in various forms was suggested as the agent,—First, in the condition of glaciers filling our valleys; next, in the condition of icebergs floating over our island, whilst under the sea; and latterly, as a great sheet or cake stretching from the Arctic regions, and overspreading the whole of northern Europe.

It is not my intention to discuss these theories, or say which appears the most probable. I allude to them now, merely to indicate the tremendous character of the agencies, which it is found necessary to invoke for the solution of the problem,—agencies all implying a very different condition of things in Scotland, as regards configuration of surface and climate, from what now prevails. These phenomena are the more interesting, because, as most of the erratic blocks lie above all the rocks, and very frequently even above the beds of clay, gravel, and sand, which constitute the surface of the land we inhabit, they indicate probably the very last geological changes which occurred in this part of the earth's surface, and which there are some grounds for supposing, may even have occurred since this country was inhabited by man.

The basis on which geologists have been obliged to build their theories, it must be admitted, is somewhat narrow. It consists merely of observations made casually by individuals, who have noticed certain appearances in districts of Scotland which they happen to have visited; and, therefore, it is little to be wondered at, that more than half a century has been required for procuring the information, scanty as it is, which has been obtained.

What appears desirable for expediting the solution of the problem, is to organise a staff of observers, and to parcel out the country amongst them, for the purpose of observing facts likely to throw light on the subject, and of making these facts known from time to time, both with a view to verification, and as a basis for further speculation.

It has occurred to me, that the numerous natural history societies and field clubs existing in Scotland, would be valuable agents in this investigation; and, moreover, that individual geologists would be pleased to co-operate in their respective districts.

I hope no one will think that the object for which I suggest this investigation, is not worthy of the trouble which it implies, and of the patronage which I ask this Society to bestow on it. These erratic blocks bear the same relation to the history of our planet, as the ancient standing or memorial stones do to the history of the early races of mankind. These last-mentioned stones,—sometimes with sculpturing on them not yet understood,—sometimes arranged in circles or other regular forms not yet explained,—sometimes found in connection with sepulture, are beheld and studied with interest, on account of the gleams of light which they throw on the people who erected them; and popular indignation justly rises, when any of these prehistoric records of our ancestors are destroyed or mutilated. The great boulder stones to which I have been referring would, if investigated and studied, in like manner cast light on the last tremendous agencies which have passed over whole regions of the earth. It is therefore important to have as many of these boulders as possible discovered and examined, and to have such of them preserved as seem worthy of study. I need not say how rapidly, during the last century, both classes of ancient stones have been disappearing; and therefore, if it be desirable to preserve the most remarkable boulders, or at all events to record their existence, and their geological features, the investigation which I advocate, cannot be too soon begun.

Alike in illustration and in recommendation of this suggestion, I will refer to an investigation for the same object commenced two years ago in Switzerland, and in the adjoining parts of France. The design was twofold,—*First*, the conservation of remarkable boulders situated on the Jura and in Dauphiny; and *second*, the recording of their positions by maps, and of their characteristic features by schedules.

With this view a circular was drawn out, and issued by the Swiss Geological Commission, pointing out the scientific bearings of the subject, and invoking the co-operation not only of provincial societies, but also of municipal authorities in the cantons, and of landed proprietors. A few extracts from the Swiss circular may not be inappropriate :—

"These erratic blocks are composed of granite, schist, or lime-
" stone; but they rest on rocks of a different description. They

" were so remarkable by their number and size, that, from an " early period, they attracted the attention of naturalists, and " suggested scientific inquiries. It is, indeed, interesting to seek " to comprehend how enormous masses, with from 40,000 to 50,000 " cubic feet of contents, and weighing from 800 to 1000 tons, could " be transported from the Alps from which they were evidently " detached, to spots 40 and 50 leagues distant, crossing deep " valleys, such as the lakes of Geneva, Neufchatel, Zurich, Con- " stance, Lucerne, &c.

" This great problem has been discussed by numerous philo- " sophers, both of Switzerland and of foreign countries." Then follows a list of names, including those of our own Playfair, Lyell, Murchison, Forbes, Tyndall, and Ramsay.

" Unhappily," (the circular goes on to state), " during the last " 100 or 150 years, these erratics have been broken up for building " purposes, and even for road metal. Recently the work of destruc- " tion has gone on more rapidly, and, unless stopped, the result " will be to obliterate all traces of one of the greatest facts in the " natural history of our country.

" Though the destruction of these blocks is now advancing with " great rapidity, there are still a number of very large specimens " left, and these the Geological Commission is anxious to pre- " serve."

" The members of Archæological Societies are interested in the " conservation of these blocks, for they often bear those curious " sculpturings, to which much importance is now justly attached."

" The lovers of legends must regret to see these blocks disap- " pearing, for ancient tradition tells how some have been flung by " the Devil on a poor hermit; that another bears the name of a " fish merchant in a town of which there is now no trace, &c.

" The Geological Commission considers that the time has come " for appealing to all who have any power over the fate of these " blocks, that is to say, to individual proprietors, to communal " authorities, and to municipalities. The Commission also entreats " natural history societies, Alpine clubs, and public bodies, to co- " operate in this work, in order to preserve for Switzerland a " feature of the country, which, if not altogether peculiar to it, is " at all events better developed there than in any other

Besides making an appeal for the conservation of boulders, the same Swiss Geological Commission suggested the propriety of marking their exact position on the Government maps.

They farther expressed a hope that these measures might reach even beyond the frontiers of Switzerland, and they referred to an offer made by a French geologist to draw up an account of the Erratics of *Souabe*, with the view of obtaining co-operation from that quarter.

A committee was appointed to carry out these views, supply the necessary schedules and maps, and conduct the correspondence.

I shall next explain what resulted from the appeal. The circular containing it was issued in the autumn of 1867, and I now quote from a report presented to the Helvetic Society of Natural Sciences at a meeting in August 1869, drawn up by Messrs Favre and Soret.

They state that, very soon after the commencement of the investigation, it was found desirable not to limit it to boulders, but to include a description of enormous heaps of gravel, existing in many districts, having the appearance of ancient moraines, and in that view likely to throw light on the mode in which the boulders were transported. Accordingly, instructions were given to indicate on the maps the position of these gravel accumulations as well as of boulders.

Messrs Favre and Soret then narrate what had been done during the previous year in the different cantons, and from their report I give the following extracts:—

In the first place, they acknowledge the liberality of Colonel Siegfried, the Director of the Federal Topographical Department, in supplying maps to assist in recording the observations.

They farther acknowledge the assistance which Colonel Siegfried had given to the investigation, by issuing instructions to the engineers surveying the slopes of the Jura, to indicate on the maps, and to describe in their reports, any remarkable erratic blocks they met with.

Reference is next made to the proceedings of the societies and clubs in the different cantons. In some of the larger cantons, as *Lucerne* and *Vaud*, the country had been divided into five and six compartments, and a small sub-committee of members had been appointed to explore each. In one of these cantons, the municipal

authorities had given orders to the inspectors of roads and bridges to aid in the investigation.

In the canton of *Zurich*, notice is taken of one remarkable block, known as the "Stone for the sacrifices *of Hegsrüti*," which had been purchased by the Society of Antiquaries, and had been brought into the town of Zurich.

In the canton of *Soleure*, blocks of enormous size, and to the number of 228, had been marked, and appointed by the municipal authorities to be preserved, these blocks being situated on lands belonging to the canton. The celebrated block of Steinhof, weighing about 1400 tons, had been purchased by means of a special subscription, and made over in property to the Helvetic Society.

Several landed proprietors are named as having gifted particular boulder stones to the societies. Thus Mr Briganti, at *Monthey*, had gifted to the Helvetic Society one block out of a remarkable group, of which I well remember the late Principal Forbes once spoke in this Society, and which I had lately an opportunity of visiting. So also Mr Bonneton of Geneva had presented to the Alpine Club of that town a piece of land, containing what is described as a magnificent boulder, and known by the name of the "Stone of Beauregard."

Even the Federal Government of Switzerland had condescended to share in what really seems to amount almost to a national movement; for reference is made to an official communication from the Chancellor, stating that the Council of State had caused an order to be issued, that all erratic blocks situated in the cantonal forests should be preserved intact, till examined by the committee.

I have had sent to me a printed report of the steps taken in the canton of *Aargau*, drawn out by Professor Mühlberg. He mentions that one of the measures taken there, was the appointment of a referee to inspect the boulders which were discovered, with the view of determining whether they were worthy of being preserved. Professor Mühlberg mentions farther, that "the State "undertakes the expense of printing and postages, as well as of "the travelling of the canton referee to the sites of the most "important boulders, and had in the meantime advanced 100 francs "to defray expenses already incurred."

These extracts from the reports, of which printed copies have

been kindly sent to me by Professor Favre of Geneva, show what is doing in Switzerland for the promotion of an object which, under the auspices of this Royal Society, I should wish to see taken up in Scotland. And before concluding what I have to say about the Swiss movement, I may refer to one circumstance which ought to be gratifying to Scotchmen, viz., that the Swiss naturalists retain a grateful recollection of what has been done by Scotchmen for exploring and making known the interesting physical features of their beautiful country. Not only have they, in specifying the names of geologists who have written on Switzerland, included all the Scotchmen who have done so, but I see in one of Professor Favre's pamphlets, written in connection with this movement, allusion to the year 1741, "when (he says) the English first pene-" trated into the valley of Chamounix,"—"and gave to that valley " a celebrity, which the previous visits of several bishops had not " obtained for it." Professor Favre records the names of these English visitors, and among them are "Lord Haddington and his " brother, Mr Baillie." The pamphlet mentioning these names I sent to the present Earl of Haddington, that he might see the courteous allusion to his ancestor; and, in returning the pamphlet, he referred me to a paragraph in Douglas's Peerage, which mentions the fact that, in the year 1740, the Earl of Haddington and his brother, George, set out on their travels to the Continent, and were for some time located with other friends at Geneva—one of these being Stillingfleet, famous in his day as a naturalist, and who in one of his works alludes to the very agreeable *reunions* of his countrymen which took place at Geneva and the neighbourhood.

I will next refer briefly to the steps taken in the south of France in co-operation with the Swiss movement. These began by a communication from Professor Favre to Mons. Belgrand, who, besides being President of the Geological Society of France, was Inspector-General of Bridges and Roads, a Government Department. This communication, which explained the object of the Swiss investigations, and also what was being done by the different cantonal societies and municipalities, was referred by Mons. Bertrand to two members, Messrs Falsan and Chantre, to report on.

It is from their report, the remarks of Mons. Bertrand upon it, and some notes of a subsequent date, published in the Transactions

of the Geological Society of France for December 1869, that I make the following extracts:—

The great interest attaching to the investigation is allowed by the reporters, and a compliment is paid to the Swiss naturalists for commencing and urging it.

Reference is made to the rapid disappearance of the boulders, and especially limestone boulders, which were generally broken up for limekilns. The reporters state that near Lyons, the greater part of the boulders had been destroyed long ago, and in particular one weighing about 150 tons, which marked the point where the boundaries of three parishes met.

Examples, however, of remarkable boulders still untouched, with legends attached to some, are specified, such as the "Pierre du Bon Dieu," of 120 tons, and the "Pierre du Diable," of 56 tons, which it is strongly recommended should, with many others of less note, be saved from destruction or injury.

Reference is then made to the steps which should be taken to carry out these views. Circulars, it is said, should be drawn up, and sent not only to the public departments which superintend the management of Government or communal lands, but also to individual landed proprietors, pointing out the scientific interest attaching to these erratic blocks.

These suggestions were at once favourably responded to and acted on. Three public departments or functionaries, viz., the Minister of Public Works, the Director-General of Forests, and the Prefects in each of the provinces of Savoy, High-Savoy, Ain, Rhone, and Isère—all adjoining Switzerland—are stated to have lent their willing co-operation.

After the project had received the approbation of the Geological Society of France, and the promise of important official support, an appeal to the friends of Natural Science was drawn up by Messrs Falson and Chantre very similar to the appeal which had been previously drawn out and issued in Switzerland. This appeal, after describing the movement and proceedings in Switzerland, proceeds thus:—"Such is the object pursued vigorously in Switzer-"land with the co-operation of departments and of individuals. "In a word, see what is going on near ourselves. Can we remain "outside of, and indifferent to, this scientific enterprise, especially

" when Mons. Favre has asked us to engage in the same work, and " to undertake for our country what he is doing for his? We are " bound to answer this appeal. The solution of the same questions " ought to occupy us. These erratic phenomena abound every- " where in our district. The debris of rocks torn from the Alps " cover the plain of Dauphiny, the plateau of the Dombes, the hills " of Croix, Rousse, and Sainte-Foy. Already many geologists " have studied these erratic phenomena in our neighbourhood, " without being able to discover a solution. The truth, when we " seek it, seems to fly from us; but we must persevere and pursue " it till it is caught.

" Our desire is simply to prevent the destruction of the most " remarkable blocks, and leave them on their natural sites, and " also to obtain a collection of specimens to illustrate them, and " we hope that our administrations will in this object not be behind " those of Switzerland and the department of Haute Savoie. Their " example would, we doubt not, be followed by individual proprie- " tors, where boulders cease to be regarded as mere masses of stone " of unusual size, but without scientific value."

Besides this appeal, printed copies of which were extensively circulated, directions and schedules were drawn out to be transmitted to local societies as well as to individuals who should undertake the investigation, in particular districts, maps of these districts being at the same time supplied.

The documents from which I have made these extracts were, as I have said, transmitted to me by Professor Favre of Geneva. He wrote to me at the same time, and concluded his letter by saying, " Voila, Monsieur, un aperçu de la marche de cette entreprise. Je " serai bien heureux, de le voir s'etendre a l'Ecosse."

In a subsequent letter he repeats his suggestion thus:—" Si vous " pouvez organiser quelque chose de semblable en Ecosse, vous " m'obligerez infiniment, en me tenant au courant."

In a third letter, he says, " Permettez moi de vous renou- " veller la demande que je vous ai addressé, en vous priant de me " tenir au courant de ce que nous ferez pour les blocs erratiques de " l'Ecosse, et des resultats que vous obtiendrez."

I have given these details of the proceedings in Switzerland and France, and quoted these passages from Professor Favre's letters,

in order both to add weight to my proposal, and show how we may proceed to attain it.

I have alluded to the existence throughout Scotland of many provincial societies whose objects are not inconsistent with the investigation which I think they may be invited to engage in. Sir Walter Elliot of Wolflee has lately been at pains to make out a list of all the Natural History Societies and Field Clubs existing in Great Britain and Ireland.

I now give this list, in so far as it applies to Scotland, in the hope that, when our proceedings are published, this list may appear in it, so that if any societies or clubs are seen to have been omitted, the omission may be taken notice of and supplied.

1. Berwickshire Naturalist's Club. (*Secretary*, Mr Geo. Tate, Postmaster, Alnwick.)
2. Hawick Archæological Society. (*Secretary*, David Watson.)
3. Tweedside Physical and Antiquarian Society.
4. Dumfries and Galloway Natural History and Antiquarian Society.
5. Edinburgh Geological Society. (*Secretary*, Geo. A. Panton, Hope Terrace.)
6. Edinburgh Naturalists' Field Club. (*Secretary*, Andrew Taylor, 5 St Andrew Square.)
7. Glasgow Natural History Society. (*President*, John Young M.D.; *Secretary*, Robert Gray, 2 Lawrence Place, Dowanhill.)
8. Glasgow Geological Society. (*President*, John Young, M.D.; *Secretary*, Dugald Bell, 136 Buchanan Street.)
9. Alloa Society of Natural History and Archæology.
10. Largo Field Natural History Society. (*Secretary*, Charles Howie.)
11. Perth Literary and Antiquarian Society.
12. Perthshire Society of Natural History. (*President*, Dr Buchanan White; *Secretary*, A. T. Scott.)
13. Montrose Natural History Society. (*Secretary*, Mr Robert Barclay.)

14. Aberdeen Natural History Society.

15. Aberdeen Philosophical Society. (*President*, Professor Ogilvie, M.D.; *Secretary*, Alex. D. Milne, 37 Thistle Street.)

16. Natural History Society, Elgin.

17. Orkney Natural History Society.

Being myself a member of one of these Societies, I know that some of its members have devoted themselves to the subject of boulders, and of moraine-looking deposits, occurring within the district over which the operations of the Society extend.

Sir Walter Elliot tells me that he has information of a Field Naturalists' Club in England which has specially directed its attention to the boulders of the district.

It is quite true that, in Switzerland and in the south of France boulders, considerable in size and numbers, are much more abundant than in Scotland, so that little searching is required to enable the provincial societies of these countries, to carry out the investigation proposed to them.

On the other hand, let it not be imagined, that in Scotland the boulders generally are not of such interest as to deserve the adoption of proceedings similar to those now being adopted in Switzerland and France. Even within the limited range of my own discoveries, I know and have measured eight boulders in the south-east of Scotland, the smallest of which is 10 tons and the largest 918 tons in weight, and all possessing features more or less significant.

There are others equally large which I have heard of, but have not seen. Moreover, almost all these boulders have old traditional names, and many of them legends which indicate, that they have been objects of popular and even superstitious regard.

There are two objects which ought to be aimed at. The first is to obtain a list of all boulders which appear remarkable; *i.e.*, remarkable for size, and instructive on account of polishing, ruts on the surface, or any other circumstance. The second is to put down on maps, a mark to represent the exact position of boulders, occurring in groups, or of large individual boulders.

Moreover, accumulations of gravel, sand, or clay in any district, in so far as they seem to have been produced by agents now no longer operating in the district, should be notified.

In order to carry out these suggestions, I would venture very respectfully to ask that the Council of this Society should pass a Special Minute expressing approval of the subject explained in this paper, and appointing a Committee of the Fellows of this Society to carry out farther proceedings. The circumstance that this Society had expressed its approval, and taken steps to aid the investigation, would alone ensure for it a favourable consideration.

The Committee would, of course, communicate with the various provincial societies throughout Scotland, by enclosing a copy of this paper or an abstract of it, and intimating readiness to send the necessary Schedules and Directions, should a willingness be expressed to enter on the investigation proposed.

I have in these remarks alluded only to the steps necessary for discovering the existence of remarkable boulders, indicating their position on a map, and obtaining a correct description of them. But the other object, which also engages attention so much in Switzerland and France, should not be lost sight of here. I allude to the conservation of boulders. The disappearance of numerous camps, buildings, standing stones, and other objects of archæological interest in all our counties, which every one now regrets, has been owing in a great measure to ignorance on the part of the proprietors and tenants on whose lands they were situated, of the value and even nature of these objects. But this work of destruction has been happily now stopped, and chiefly by the interference and influence of our Society of Antiquaries. In like manner, the demolition of Boulders which has been going on rapidly in Scotland, will, I hope, be arrested, when the proprietors and tenants on whose lands they stand, are made aware of the interest they excite, and of what is being done to preserve them in other countries. Of course, it would only be certain boulders which it would be desirable to preserve, boulders remarkable for size, or shape, or position, or for markings upon them; and when a report was made to the Committee of any boulder of this description, the Committee would judge whether an application should be made to the proprietor on whose lands it was situated, to spare the stone, so that it

might be preserved for examination and study. I have little doubt that such an appeal would be attended to. Indeed, in the great majority of cases, a proprietor would be pleased to learn, that an object of scientific interest had been discovered on his estate, and would be glad to have it in his power to accede to any request in relation to it coming from a Committee of this Society.

With regard to the mode of meeting the expenses attending the investigation and other proceedings suggested in this paper, it occurs to me that subscriptions from individuals should be chiefly relied on, and that the Council of this Society should only promise such aid as the state of the Society's funds and their appreciation of the proceedings of the Committee, may suggest to them. The Committee will, no doubt, make a Report at least once a year of their proceedings, which the Council may allow to be read at a meeting of the Society, if its contents were sufficiently interesting.

3. Note of a New Form of Armature and Break for a Magneto-Electric Machine. By R. M. Ferguson, Ph.D.

The magneto-electric machine, which I am about to describe, approximates in its general arrangements to Ladd's hand-machine. In it Mr Ladd makes use of a compound Siemens' armature, consisting of two separate armatures placed in length, and revolving round the same axis, with their coils at right angles to each other. The armature revolves between the poles of an electro-magnet, of the description introduced by Mr Wilde. The electro-magnet, in the present instance, is made of a rectangular piece of boiler-plate, three-quarters of an inch in thickness, bent so as to form three sides at right angles to each other, as shown (in section) in fig. 1. The upright sides (P P′ P) are nearly 9 inches high and 11 inches in length, and the top of the same length is 6 inches broad. Pieces of cast-iron (N and S) are put in the open end to form the poles of the magnet. About 300 yards of a double No. 14 wire, wrapped round the upright sides, make the coil (C C C C) of the electro-magnet. One of the armatures in Ladd's machine furnishes a current to the coil of the electro-magnet; the other gives out an external current. To distinguish the two, the counterparts of which occur in the arrangement I bring before you, I shall call the first the inter-

nal current, and the second the external current; and the coils furnishing them I shall designate the magnetic coil and the electric coil respectively. The action of the magnetic coil is based on Siemens' and Wheatstone's principle of reciprocal increase. When a Siemens' armature revolves between the poles of an electro-magnet, what feeble magnetism there may be in the iron core generates a feeble current in the armature coil. This current, by a commutating arrangement of revolving collar and springs, is sent into the coils of the electro-magnet, which in consequence rises in

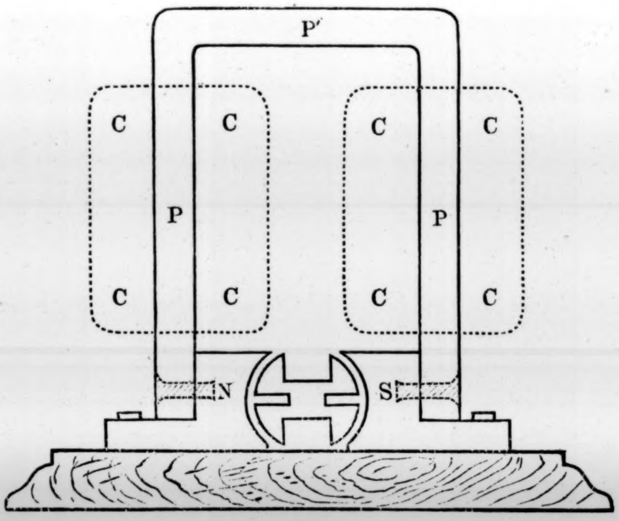


Fig. 1.

power. It is now able to excite a stronger armature current, thereby rendering itself still more powerful, and this mutual action goes on until the driving force is insufficient to continue the action. Ladd has ingeniously turned this principle to account in his machine, the magnetic coil of which furnishes electricity for the electro-magnet, and this last is thereby rendered competent to generate electricity in the electric coil available for external use.

Wishing to make a machine to give off a current equal to a few cells of Bunsen, I thought of trying the following deviation from Ladd's construction:—Instead of having two separate armatures revolving on the same axis, I thought one might serve, in which two coils were inserted, the one at right angles to the other. In the revolution of a Siemens' armature there are two polarities, so

to speak, one only of which is utilised, viz., that which takes place (fig. 2) when the greatest length of the iron core lies in the line joining the two poles; the other polarity ensues when this main axis is perpendicular to the line of poles (fig. 3). This second

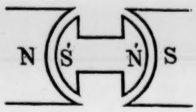


Fig. 2.

Fig. 3.

polarity is, from the less favourable position of the core, necessarily weaker than the first; but it struck me that it might be quite sufficient to furnish the internal current, leaving to the more powerful polarity the task of generating the external current. Another advantage seemed to flow from this utilisation. When an armature without coil or closed circuit revolves within a magnet, the energy expended in its motion heats its particles. When the core is provided with a coil and closed circuit, part of this energy, instead of assuming the form of heat, is transmuted into the energy of an electric current, and the electricity induced is so much deducted from the heat that would otherwise appear in the armature. In the ordinary construction the weaker polarity, being unprovided with a coil, results only in heat; but if it be furnished with such, as in the arrangement I suggest, and its molecular energy thereby tapped, so to speak, the heat of the armature may be partially withdrawn in the shape of an electric current. A current sufficient to magnetise the electro-magnet may thus be got, for no additional expenditure of force, but only by the conversion of heat that would otherwise be mere waste, so far as the action of the machine was concerned. When one of Wilde's small machines, in which a battery of permanent magnets is used instead of an electro-magnet, is turned by the hand, additional resistance is felt on the armature circuit being closed more especially by a short wire. The current got from the armature would thus seem to be formed partially from the conversion just mentioned, and partially from a new access of force demanded by the creation of the current. In the arrangement I here describe, a different action takes place, for when the coil of the electro-magnet is disjoined from the magnetic coil and included in the circuit of a single Bunsen cell, the feeling

of diminished resistance is nearly as decidedly felt as that of increased resistance in Wilde's machine on closing the electric coil circuit. The same feeling is not so decided in the case of the magnetic coil, and this, no doubt, arises from its smaller dimensions; at any rate, there is no additional force needed. Whether this action has its origin in an essential difference in the action of permanent magnets and electro-magnets in these circumstances, or in some peculiarity of construction, is immaterial to the present inquiry, for to all appearance the armature currents cost no additional energy, but are got entirely from the waste heat of the armature.

The core of the armature (fig. 4 *a*) is 11 inches long and $2\frac{1}{2}$ inches in diameter. The main longitudinal cut or groove is $1\frac{3}{8}$ inch wide and $\frac{1}{2}$ inch deep. The small cut is $\frac{3}{8}$ of an inch wide and $\frac{3}{4}$ of an inch deep.* In the large cut is wound the electric coil, consisting of a cable of 8 silk-insulated wires, $\frac{1}{40}$ of an inch in diameter, and 82 feet long. The magnetic coil in the small cut is made of a cable of four such wires, 46 feet in length. The electric coil thus contains about four times as much wire, and offers about the same electric resistance as the magnetic coil.

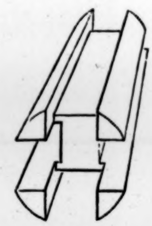


Fig. 4 *a*.

The two grooves leave four protruding ends at each end of the armature. To these are screwed a bronze cap and spindle of revolution (figs. 4 and 5, which are on a larger scale than fig. 4 *a*).

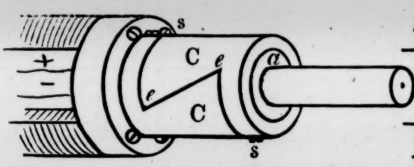


Fig. 4.

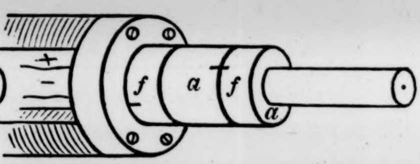


Fig. 5.

A collar of wood (*a*) is fixed next to the spindle, and on this collar two ferrules of iron (*f f* fig. 5) are put, separated by the wood to prevent contact. To these ferrules the wires from the coils (+ −) are soldered, care being taken to prevent unnecessary contact. A cylindrical collar (C C fig. 4) turns on the ferrules, and can be turned round and fixed in any position by screws (*s s* fig. 4). The collar is made up of three parts, two pieces of iron (one is shown

* In the figure both cuts to be shown clearly appear of the same size.

in fig. 7) cut out of the same tube and kept from touching, by being fixed to a vulcanite ferrule (*v* in fig. 6, which shows the inside of half the collar) placed inside and between them. The ends of the iron pieces slide on the iron ferrules beneath, and are in conducting connection with them. Electrical contact is made by springs pressing on this composite collar, and which are metallically connected with the binding screws, the poles of the armature coils. The collar and springs at each end form the breaks or commutating arrangement of their respective coils. The cross line of separation (*e e* fig. 4) can be fixed in any position, and currents in one or different directions thereby obtained in the course of a revolution. The pressure of the springs against the collars is regulated by screws.

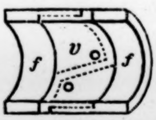


Fig. 6.

Fig. 7.

When the machine is prepared for working, the cross lines of the commutating collar of the magnetic coil are placed at right angles to the plane of the coil, the position of maximum effect. If the handle of the machine be turned when the circuit of the electric coil is open, one or two turns bring the hand of the operator to something like a dead halt; the resistance to further motion is so great as to challenge its continuance. If, now, the external circuit be closed, immediate relief is felt, as if part of the internal current had been diverted into the external circuit from the coils of the electro-magnet. The relief thus experienced, moreover, bears some proportion to the conductivity of the external circuit. With an easy circuit, the work expended in turning the handle is easy; with a resisting circuit, the driving resistance becomes correspondingly great. The hand is thus made to sympathise with the nature of the external circuit, and the experimenter feels as if he were charged mechanically with a resistance offered electrically. Suppose, for instance, we have a piece of thin wire to heat or melt; at first little or no driving resistance is felt, but the moment that the wire begins to get hot, the arm becomes charged with a heavy resistance, which grows as the wire rises in temperature till it melts, and then suddenly the excessive no-circuit

resistance is felt. The moment that there is hard work to be done in the external circuit, the strength of the arm is put to the proof. When water is decomposed by the machine, the strain upon the arm does not rise beyond a certain amount, at whatever speed the handle be driven. In working an induction coil, the load on the arm appears capable of rising to any extent, and the length or density of the spark bears something like a proportion to the burden of work. With an electric resistance great enough, and an inexhaustible driving power, there seems no limit to the electric effect attainable, and that, too, with little increase of speed.

When a tangent galvanometer is interposed in the external circuit, something may be learned of the way this takes place. With an easy circuit, where little difficulty is felt in driving, a current of about 60° may be got. When a thin wire is now interposed, the needle does not reach this point, for the wire (iron wire $\frac{1}{80}$ inch in diameter) melts or ignites between 30° and 40°, and yet while the heating lasts the strain is enormously greater than before. If the galvanometer be inclosed in the internal circuit, and the wire melted in the electric circuit, just at the point when the heating begins, the needle takes a sudden swing upwards. Thus, if it be at 20° before the heating sets in, it will rise to 30°, and stay there till the wire melts, when, if the motion be continued, it again takes a start upwards. If the magnetic coil be detached from the coil of the electro-magnet, and if its function be performed by one Bunsen cell, this increase of load is not felt, a greater effect in the external circuit being only attainable by an increase in velocity, and the same holds with a battery of permanent magnets.

That two separate coils, by being imbedded in the same piece of iron, should thus act upon each other seems strange. One might almost think that it arose from the particles of iron refusing to polarise and unpolarise quick enough. The maximum speed of revolution of the armature is about 2500 times a minute. The driving gear multiplies 22 times, so that this speed is nearly as much as the arm can effect. A particle of iron would have thus 10,000 times to polarise and unpolarise in a minute. A little consideration will show, however, that it is from no such incapacity on the part of the iron; for at the same rate of revolution, the two effects are felt with the different circuits. Speed in these cases, therefore, has not

overshot the mark. The cause of the action appears to me as follows:—When the line of the armature (fig. 8) is vertical—when, in fact, the strongest action is taking place in the small coil—the wires of the large coil cut the lines of magnetic force between N and S at right angles, the best time and the best place for a current to be induced in them. Although, then, the longitudinal polarity of the iron has disappeared, the coil takes up the action and makes a north and a south end, even when the main line of the armature is upright, and should be free from polarity. This coil induction or polarity is feeble, contrasted with that resulting through the iron, and would have little effect if the coils were near each other in size. It is only in the present case, where there is such a disparity between the coils, that the interference grows to a sensible amount. In support of this view of the matter, it may be mentioned that when the larger coil is connected with the electro-magnet, little relief is felt on an easy circuit being made for the smaller coil. The effect of the interference is to lessen the current induced in the smaller coil. A particle at *a*, for instance (fig. 8), which when left to the action of the poles of the electro-magnet would give its full quota of electric induction, is by the cross polarity magnetically forced round, so to speak, into a less favourable position for doing so. But how is this interference stopped by a resisting external circuit? In this way, I imagine. The available electro-motive power may take the form of large quantity in an easy circuit, or little quantity in a resisting circuit. On consulting the galvanometer in a resisting circuit, while the strength is taxed to the utmost, the current is often found weak. It is the quantity of electricity that is the cause of the interference, and not the work value of the circuit. When the strength of the electric current is great with a resisting circuit, that of the magnetic current has been proportionally exalted.

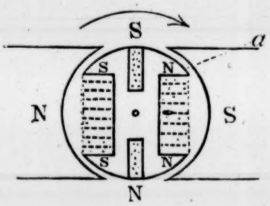


Fig. 8.

The interference of the two coils with each other can be shown in a simple way. When the coil of the electro-magnet is detached from the magnetic coil and joined up with a Bunsen cell, we have, on turning the handle, both armature coils prepared to give ex-

ternal currents. If, in the circuit of the electric coil, a few inches of fine platinum wire be included, and the circuit of the magnetic coil half completed, so that one end of the connecting wire has only to touch the other binding screw to close it, and the handle be put in sufficient motion, the platinum wire becomes white hot, and this sinks to a dull red when contact in the magnetic circuit is made. The same takes place when the coils are reversed. Such an action as this suggests the supposition that what appears in the second coil is but electricity stolen from the first, and that the arrangement effects only a convenient distribution, and not an increase of the electricity available. I cannot, with the observations I have yet made, say that such is not true in all cases, but in one case, at least, the only one I have examined, such a supposition cannot be entertained, and that is when both coils work together in the same circuit. When both coils, as just mentioned, are ready to give external currents under the magnetism induced by one Bunsen cell, it is quite possible, by accustoming the ear to the note produced by the springs rubbing on the revolving collars, to get the arm to work at a uniform speed. If the cell be steady, you can, within a fraction of a degree, produce the same angle in the galvanometer in the same circumstances. I have made repeated observations in this way as to what current the electric coil would give when acting alone, as to what the magnetic coil would give, and as to what both together would effect. The circuits in these cases consisted of the coils themselves and the wires leading to a tangent galvanometer some 12 feet off, and the working of the machine and the observing of angles were done by different persons. The resistances in both circuits were sensibly the same. The resistance of the electric coil was 32 inches of a German silver wire in my possession, that of the magnetic coil 34, and that of the galvanometer wire 5 inches. To these must be added the resistance introduced by the imperfect contact of the break-springs, which, at a high speed, and especially in the case of the machine exhibited where the armature is not quite truly centred, must be considerable. The difference between the two coils would thus almost disappear on the total resistances of their respective circuits. This being the case, the work value of the electricity appearing in each will be as the squares of the tangents of the angles observed. Now, in all the

observations I have made, the sum of these for the two coils separately was approximately equal to that obtained when both currents were sent into the galvanometer circuit. To give an idea of how nearly this comes out, I may cite one observation repeated three times in succession with the same result. I found the angle of both together to be $47\frac{1}{2}°$, that of the electric coil separately 40°, and that of the magnetic coil separately 34°. Now the square of the tangent of $47\frac{1}{2}°$ is 1·1909, and the sum of those of the other two 1·15905.

The theory of the machine, as I understand it, may be thus shortly summed up. In one case, namely, that of an easy common circuit, and it is likely to be more or less so in all cases, the two coils contribute each their full quota to the total electric fund of the armature. When the resistance of the circuits differ, this fund is divided inversely in some function of the relative resistance, but whether this takes place so as to excite the electro-magnet at no original expense of driving energy is still a matter for further determination. The results got from the machine would lead us to suspect as much, for they compare favourably with machines where a permanent battery of magnets is used; but this test, though so far satisfactory, is far from exact.

The interference of the coils seems to me to be a hopeful feature of the arrangement, as it does not make increased power simply dependent on increased velocity. There is a promise in it that by adjusting the relative sizes of the coils a powerful current may be got at a really practicable speed, and there would thus be obviated the serious objection to this class of machines, which, however astonishing in their power, are apt to wear themselves out by their rapid rate of motion when kept in action for days together. Even in the machine before you, if the collars were properly turned and centered, so as to give good contact with the springs at all rates of revolution, I have reason to believe that its effective speed of revolution would be very much diminished.

In mentioning what a machine like this can do, considerable latitude must be understood in interpreting results. The strength or ardour of different workers may tell very differently. The only fair way would be to give the electric effect corresponding to a weight falling so far per second, but this involves opportunities of

experiment which I have not at my command. When I say that 6 inches of soft iron wire $\frac{1}{80}$ of an inch in diameter can be melted or ignited by it, I only mean to say that the arm of an ordinary man, working briskly for a second or two, can accomplish this, though it would be hard work for him to continue the same for a minute. A stronger arm than usual, or a more ardent labourer, would do much more than this. A battery of six Bunsen cells, each with an effective surface of 42 square inches, melted 5 inches of the same wire. With an induction coil a spark of $1\frac{1}{2}$ inches can be got with an expenditure of labour that may be continued for a minute or two; with intense exertion a spark of 5 or even more inches may be got. By working reasonably for a minute from $2\frac{1}{2}$ to 4 cubic inches of explosive gas can be got from a voltameter; working very hard for a quarter of a minute at the rate of 6 inches or more may be obtained. To turn a handle some 100 times a minute, more especially against some resistance, is not work that can be easily continued for minutes; and such machines, when driven by the hand, are only good for incidental, not continuous use. To keep down the pull on the hand with a resisting circuit, the commutating collar of the magnetic coil has to be turned round from its position of maximum effect. There is a certain speed at which the hand can best work, for slow and difficult motion is not so convenient nor attended by so good results as quick and easy motion.

The machine is well adapted for an educational instrument, viz., for illustrating electro-magnetic action. If the electro-magnetic coil be joined with one cell of Bunsen, and the electric coil with five or six cells, the conditions of the machine are reversed; and now electricity produces motion, instead of motion producing electricity. The handle is made to go round with considerable velocity, and if the belt that connects the gearing with the handle be removed, the armature alone spins round at a great rate. If now the poles of the magnetic coil be joined, the armature instantly slows, and the slowing is all the more marked the less the resistance of the circuit offered. The current of this new circuit can raise to a white heat about a $\frac{1}{4}$ inch of fine platinum wire. It may be worth mentioning, that the current given off by the magnetic coil under these conditions is singularly steady, and that its strength is something like inversely proportional to the circuit resistance. This slowing of

the armature seems at variance with what I have stated before, that less instead of more driving resistance is felt in closing either of the armature circuits, for here the new current seems to be paid for out of the motion of the armature. The discrepancy may possibly be accounted for by the consideration that both coils are now antagonistic in their action, and that whatever part of the induced current appears in the magnetic coil, from whatever source derived, goes directly to oppose the conditions favourable to motion, and that between the opposing actions more heating in the core may be the accompaniment or equivalent of slower motion. When the coil of the electro-magnet is joined with the larger (electric) coil, so that a wire has only to touch the unconnected binding screw to close the circuit, and when the arm puts the machine into rapid motion, it is brought to an instant, one might say an impotent halt, on the wire touching the binding screw. One cannot help thinking, in trying such an experiment, that coil-brakes or drags may be yet extensively used in machinery.

Whether this machine be any improvement or even a rival to existing machines, I do not pretend to say. I only wish in this paper to bring the peculiarities of its action before the notice of the Society.

4. Mathematical Notes. By Professor Tait.

1. On a Property of Self-Conjugate Linear and Vector Functions.

In the course of an investigation connected with the free rotation of a rigid body I was led to the remark that, if ξ and η be two vectors related to one another so that

$$\xi = \mathrm{V}.\eta\phi\eta \,,$$

where ϕ is a self-conjugate linear and vector function, we have also

$$\eta = \mathrm{V}.\xi\phi\xi \,,$$

(so that the relation is reciprocal) provided

$$\mathrm{S}.\eta\phi\eta\phi^2\eta = 1 \,,$$

which implies also the corresponding equation

$$\mathrm{S}.\xi\phi\xi\phi^2\xi = 1 \,.$$

The surface of the third order, represented by either of the two latter equations, is well known, and the property above shows a curious relation between certain of its vectors and those of a central surface of the second order. It has also interesting applications to the lines of curvature of the surface.

If ξ and η be unrestricted, the theorem above may be put in the more general form that the two following equations are consequences one of the other, viz.:—

$$\frac{\xi}{S^{\frac{1}{3}}.\xi\phi\xi\phi^2\xi} = \frac{V.\eta\phi\eta}{S^{\frac{2}{3}}.\eta\phi\eta\phi^2\eta},$$

$$\frac{\eta}{S^{\frac{1}{3}}.\eta\phi\eta\phi^2\eta} = \frac{V.\xi\phi\xi}{S^{\frac{2}{3}}.\xi\phi\xi\phi^2\xi},$$

From either of them we obtain the equation

$$S\phi\xi\phi\eta = S^{\frac{1}{3}}.\xi\phi\xi\phi^2\xi\, S^{\frac{1}{3}}.\eta\phi\eta\phi^2\eta,$$

which, taken along with one of the others, gives a singular theorem when translated into ordinary algebra.

2. Relation between corresponding Ordinates of two Parabolas.

Two projectiles are anyhow projected simultaneously from a point, what is the relation between their vertical heights at any instant?

This simple inquiry, which was instituted in consequence of some results recently obtained from thermo-electric experiments (see *ante*, p. 311) carried on at high temperatures, where the indications given by two separate circuits, immersed in the same hot and cold bodies, were used as ordinate and abscissa, leads to a very curious consequence.

Let

$$x = At(B - t)$$

and

$$y = A't(B' - t)$$

be any two parabolas whose axes are vertical, and which pass through the origin. We have

$$y = \frac{A'x - Ay}{A(B - B')}\left[B - \frac{A'x - Ay}{AA'(B - B')}\right].$$

or

$$(A'x - Ay)^2 = AA'(B' - B)(ABy - A'B'x).$$

This, again, is the equation of a parabola, which passes, like the others, through the origin, but whose axis is no longer vertical.

The converse suggests another easy but interesting problem.

If we write ξ for $\frac{x}{A}$, η for $\frac{y}{A'}$, and f and f' for the halves of B and B′, we easily see that the last equation above becomes

$$(\xi - \eta)^2 = 4\overline{f - f'}\,(f'\xi - f\eta).$$

Every parabola passing through the origin may have its equation put in this form. Hence, as ξ and η are dependent on one another (in the thermo-electric as in the projectile case) only as being both functions of temperature, or of time, it is obvious that we must seek to break this expression up into a linear relation between functions of ξ and η separately. A well known transformation leads to

$$\sqrt{f^2 - \xi} - \sqrt{f'^2 - \eta} = \pm(f - f').$$

whence

$$\sqrt{f^2 - \xi} = \pm(\tau - f),$$

$$\sqrt{f^2 - \eta} = \pm(\tau - f'),$$

where τ is some function of time or of temperature. These give

$$\xi = \tau(2f - \tau),$$

$$\eta = \tau(2f' - \tau).$$

Hence, in the thermo-electric case, if we obtain a parabola by using, as ordinate and abscissa, the simultaneous indications of any two circuits whose junctions are at the same temperatures, and if one of them gives a parabola (with axis vertical) in terms of absolute temperature, τ must be a linear function of the difference of absolute temperatures of the junctions, and, therefore, the other circuit gives a similarly situated parabola in terms of the absolute temperature.

3. On some Quaternion Transformations.

(*Abstract.*)

Since the algebraic operator

$$\epsilon^{h\frac{d}{dx}},$$

when applied to any function of x, simply changes x into $x+h$, it is obvious that if σ be a vector not acted on by

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

we have

$$\epsilon^{-S\sigma\nabla} f(\rho) = f(\rho + \sigma),$$

whatever function f may be.

If Δ bear to the constituents of σ the same relation as ∇ bears to those of ρ, and if f and F be any two functions which satisfy the commutative law in multiplication, this theorem takes the curious form

$$\epsilon^{-S\Delta\nabla} f(\rho)\,F(\sigma) = f(\rho + \Delta)\,F(\sigma) = F(\sigma + \nabla)\,f(\rho);$$

of which a particular case is

$$\epsilon^{\frac{d^2}{dxdy}} f(x)\,F(y) = f\left(x + \frac{d}{dy}\right)F(y) = F\left(y + \frac{d}{dx}\right)f(x).$$

The modifications which the general expression undergoes, when f and F are not commutative, are easily seen and need not be indicated in this abstract.

If one of these be an inverse function, such as for instance may occur in the solution of a linear differential equation, these theorems of course do not give the arbitrary part of the integral, but they often materially aid in the determination of the rest.

Other theorems are given, involving operators such as $\epsilon^{S\rho\nabla}$, $\epsilon^{S.\alpha\rho\nabla}$, &c. &c.

The paper contains numerous applications, extensions, and interpretations of these fundamental theorems.

But there are among them results which appear startling from the excessively free use made of the separation of symbols. Of

these I now give but one, which, however, with that in the succeeding Note, is quite sufficient to show their general nature.

Let P be any scalar function of ρ. It is required to find the difference between the value of P at ρ, and its *mean* value throughout a very small sphere, of radius r and volume v, which has the extremity of ρ as centre.

From what is said above, it is easy to see that we have the following expression for the required result:—

$$\frac{1}{v}\iiint\left(\epsilon^{-S\sigma\Delta}-1\right)\mathrm{P}d\varsigma.$$

where σ is the vector joining the centre of the sphere with the element of volume $d\varsigma$, and the integration (which relates to σ and $d\varsigma$ alone) extends through the whole volume of the sphere. Expanding the exponential, we may write this expression in the form

$$-\frac{1}{v}\iiint\left(S\sigma\nabla-\frac{1}{2}(S\sigma\nabla)^2+\ldots\right)\mathrm{P}d\varsigma$$

$$=-\frac{1}{v}S.\nabla\mathrm{P}\iiint\sigma\,d\varsigma+\frac{1}{2v}\iiint(S\sigma\nabla)^2\mathrm{P}d\varsigma-\&c.,$$

higher terms being omitted on account of the smallness of r, the limit of $T\sigma$.

Now, symmetry shows at once that

$$\iint\sigma\,d\varsigma=0.$$

Also, whatever constant vector be denoted by α,

$$\iiint(S\alpha\sigma)^2d\varsigma=-\alpha^2\iiint(S\sigma\,U\alpha)^2d\varsigma.$$

Since the integration extends throughout a sphere, it is obvious that the integral on the right is half of what we may call the moment of inertia of the volume about a diameter. Hence

$$\iiint(S\sigma U\alpha)^2d\varsigma=\frac{vr^2}{5}.$$

If we now write ∇ for α, *as the integration does not refer to* ∇, we have by the foregoing results (neglecting higher powers of r)

$$\frac{1}{v}\iiint\left(\epsilon^{-S\sigma\nabla}-1\right)\mathrm{P}d\varsigma=-\frac{r^2}{10}\nabla^2\mathrm{P},$$

which is the expression given by Maxwell (*London Math. Soc. Proc.*, Vol. III., No. 34, 1871). Although, for simplicity, P has here been supposed a scalar, it is obvious that in the result above it may at once be written as a quaternion.

4. On an Expression for the Potential of a Surface-distribution, and on the Operator $T\nabla = \sqrt{\left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2}$.

If ρ be the vector of the element ds, where the surface density is $f\rho$, the potential at σ is

$$\iint ds f\rho\, FT(\rho - \sigma),$$

F being the potential function, which may have any form whatever.

By the preceding Note this may be transformed into

$$\iint ds f\rho\, \epsilon^{S\sigma\nabla} FT\rho;$$

or, far more conveniently for the integration, into

$$\iint ds f\rho\, \epsilon^{S\rho\Delta} FT\sigma,$$

where Δ depends on the constituents of σ in the same manner as ∇ depends on those of ρ.

A still farther simplification may be introduced by using a vector σ_0, which is finally to be made zero, along with its corresponding operator Δ_0, for the above expression then becomes

$$\iint ds\, \epsilon^{S\rho(\Delta - \Delta_0)} f\sigma_0 FT\sigma,$$

where ρ appears in a comparatively manageable form. This is the expression to which the title of the Note refers. It is obvious that, so far, our formulæ are applicable to any distribution. We now restrict them to a superficial one.

Integration of this last *form* can always be easily effected in the case of a surface of revolution, the origin being a point in the axis. For the expression, so far as the integration is concerned, can in that case be exhibited as a single integral

$$\int_p^q dx\phi x \epsilon^{ax},$$

where φ may be any scalar function, and x depends on the cosine of the inclination of ρ to the axis. Now

$$\int_p^q dx\epsilon^{ax} = \frac{1}{a}\left(\epsilon^{qa} - \epsilon^{pa}\right),$$

and by operating by $\left(\frac{d}{da}\right)$ we have

$$\int_p^q dx x^m \epsilon^{ax} = \left(\frac{d}{da}\right)^m \frac{\epsilon^{qa} - \epsilon^{pa}}{a},$$

so that

$$\int_p^q dx\varphi x\epsilon^{ax} = \varphi\left(\frac{d}{da}\right) \cdot \frac{\epsilon^{qa} - \epsilon^{pa}}{a}.$$

But (as the interpretation of the general results is a little troublesome) I confine myself at present to the case of a spherical shell, the origin being the centre and the density unity, which, while much simpler, sufficiently illustrates the proposed mode of treating the subject. I hope to return to the question, and to develope at length the proposed mode of solution.

We easily see that in the above simple case, α being any constant vector whatever, and a being the radius of the sphere,

$$\iint ds\, \epsilon^{S\alpha\rho} = 2\pi a \int_{-a}^{+a} \epsilon^{x T\alpha} dx = \frac{2\pi a}{T\alpha}\left(\epsilon^{aT\alpha} - \epsilon^{-aT\alpha}\right).$$

Now, it appears (though I cannot say that I am yet quite satisfied with the *logic* of any of the proofs that have occurred to me) that *we are at liberty to treat* Δ *as* α *has just been treated.* It is necessary, therefore, to find the effects of such operators as $T\Delta$, $\epsilon^{aT\Delta}$, &c., which seem to be novel, upon a scalar function of $T\sigma$; or T, as we may for the present call it.

Now

$$(T\Delta)^2 F = -\Delta^2 F = F'' + \frac{2F'}{T},$$

whence it is easy to guess at a particular form of $T\Delta$. To be sure that it is the only one, assume

$$T\Delta = \varphi \frac{d}{dT} + \psi,$$

where φ and ψ are scalar functions of T to be found. This gives

$$(\mathrm{T}\Delta)^2\mathrm{F} = \left(\varphi\frac{d}{d\mathrm{T}} + \psi\right)\left(\varphi\mathrm{F}' + \psi\mathrm{F}\right)$$

$$= \varphi^2\mathrm{F}'' + (\varphi\varphi' + \psi\varphi + \varphi\psi)\mathrm{F}' + (\varphi\psi' + \psi^2)\mathrm{F}.$$

Comparing, we have

$$\varphi^2 = 1,$$

$$\varphi\varphi' + \psi\varphi + \varphi\psi = \frac{2}{\mathrm{T}},$$

$$\varphi\psi' + \psi^2 = 0.$$

From the first,

$$\varphi = \pm 1,$$

whence the second gives

$$\psi = \pm\frac{1}{\mathrm{T}},$$

and the third shows that the *upper* sign must be taken. That is

$$\mathrm{T}\Delta = \frac{d}{d\mathrm{T}} + \frac{1}{\mathrm{T}}.$$

Also, an easy induction shows that

$$\left(\mathrm{T}\Delta\right)^n = \left(\frac{d}{d\mathrm{T}}\right)^n + \frac{n}{\mathrm{T}}\left(\frac{d}{d\mathrm{T}}\right)^{n-1}.$$

Hence we have at once

$$\epsilon^{a\mathrm{T}\Delta} = 1 + a\left(\frac{d}{d\mathrm{T}} + \frac{1}{\mathrm{T}}\right) + \cdots + \frac{a^n}{1.2\ldots n}\left[\left(\frac{d}{d\mathrm{T}}\right)^n + \frac{n}{\mathrm{T}}\left(\frac{d}{d\mathrm{T}}\right)^{n-1}\right] + \&c.$$

$$= \epsilon^{a\frac{d}{d\mathrm{T}}} + \frac{a}{\mathrm{T}}\epsilon^{a\frac{d}{d\mathrm{T}}},$$

so that

$$\epsilon^{a\mathrm{T}\Delta}\mathrm{F}\mathrm{T}\sigma = \frac{\mathrm{T}\sigma + a}{\mathrm{T}\sigma}\mathrm{F}(\mathrm{T}\sigma + a).$$

In using such a formula we must carefully remark that F is defined as a function of a *tensor*, *i.e.*, of *a quantity essentially positive*, so that should a be negative and of greater magnitude than $\mathrm{T}\sigma$ the quantity of which F is a function becomes $a - \mathrm{T}\sigma$.

Hence, putting Δ for a in the integrated result above, we have for the potential at σ

$$\frac{2\pi a}{T\Delta}\left(\epsilon^{aT\Delta} - \epsilon^{-aT\Delta}\right)FT\sigma .$$

That this may be constant, $= 2\pi a V$ suppose, so long as $T\sigma \nless a$, we must have

$$(T\Delta)V = \frac{2}{T\sigma}\Big[(T\sigma + a)F(T\sigma + a) - (T\sigma - a)F(a - T\sigma)\Big];$$

which gives at once

$$V = \text{constant} = 2\text{, suppose,}$$

and

$$Fx = \frac{1}{x},$$

the gravitation law.

And as the operator becomes, for $T\sigma \ngtr a$, by expansion

$$2a\left\{1 - \frac{a^2\Delta^2}{1.2.3} + \frac{a^4\Delta^4}{1.2.3.4.5} - \dots\right\}$$

while

$$\Delta^2 \frac{1}{T\sigma} = 0,$$

we have for the external potential the usual expression

$$\frac{4\pi a^2}{T\sigma}.$$

5. An Experimental Research on the Antagonism between the Actions of Physostigma and Atropia. By Dr Thomas R. Fraser. (With a diagram.)

(*Abstract.*)

In a Preliminary Note, read before this Society on the 31st of May 1869 (see *Proceedings*), a number of experiments were described, which proved that the lethal action of certain doses of physostigma can be prevented by the administration of atropia.*

* June 1871.—While this Abstract is passing through the press, the author has received a paper by M. Bourneville, in which the above result is satisfactorily confirmed by experiments on guinea-pigs.

Further, it was pointed out, that antagonism between any two substances, in the sense of the lethal action of the one being preventible by the physiological action of the other, had not previously been shown to exist by any certain and satisfactory evidence. In the various instances where experiment seemed to indicate the existence of such an antagonism, sufficient proof was not given that the dose of the substance whose action appeared to be antagonised was certainly a lethal one. The conflicting opinions and doubts this fallacy has given origin to, have induced the author to follow a plan whereby it may be completely avoided.

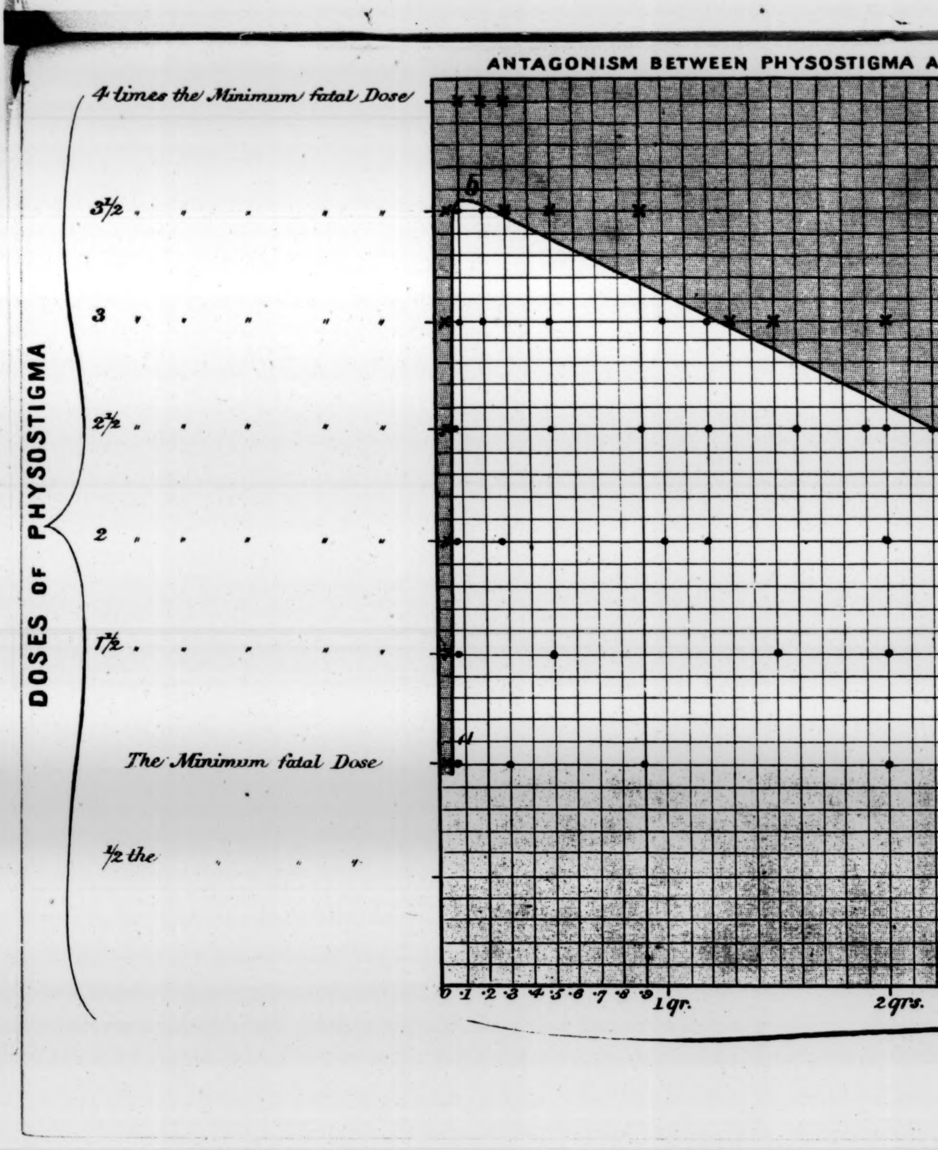
In the first place, the minimum fatal dose of physostigma for the species of animal employed was accurately determined by a number of preliminary experiments; so that the weight of the animal being ascertained, it was an easy matter to be certain of the dose that could kill it. Then, in those experiments where an animal recovered after the administration of a dose of atropia given in combination with a dose of physostigma, equal to or in excess of the minimum fatal, it was killed many days afterwards, and when the effects of the two substances had completely disappeared, by a dose of physostigma, equal to or less than that from which it had previously recovered. *Therefore, when the administration of atropia prevented an otherwise fatal dose of physostigma from causing death, a perfect demonstration was obtained of the power of atropia to produce some physiological action or actions that counteracted some otherwise lethal action or actions of physostigma.*

In the preliminary note referred to, it was suggested that, as both atropia and physostigma are capable of producing a number of different actions, several of which may not be mutually antagonistic, and that, as both substances are capable of producing several actions of a similar kind, considerably less potent to cause death than those by which their fatal effects are usually induced, it would probably be found that a region exists where the non-antagonised and the similar actions are present in sufficient degrees of activity to be themselves able to produce fatal results. This anticipation has proved to be correct. A large number of experiments have been made, by which the region of the successful antagonism of fatal doses of physostigma has been defined with considerable exactness. The smallest and the largest doses of atropia that are able to pre-

vent death after the administration of different fatal doses of physostigma, and the maximum fatal dose of physostigma that is capable of being rendered non-fatal by atropia were ascertained, and it was found that beyond these various points death may be produced by combined doses of the two substances, either by some non-antagonised action belonging to one or other of them, or by a combination of similar actions belonging to both.

As the above results could be obtained only by performing a very large number of experiments, rabbits were the animals selected, it being impossible to obtain a sufficient number of dogs, or other convenient animal. The weight of animal employed was, as nearly as possible, three pounds; and when below or in excess of this a correction was made, so that each dose represented three pounds weight of animal.

In one portion of this investigation, experiments were performed in which physostigma was given five minutes after atropia, both substances being injected under the skin. In the first series, the dose of physostigma was the minimum fatal, and the doses of atropia ranged from one that was too small to prevent the lethal action of this dose of physostigma, through a number of gradually increasing doses of atropia that were able to prevent death, until a dose was found whose administration resulted in death. Similar series of experiments were made with doses of physostigma one and a-half times, twice, two and a-half times, thrice, and three and a-half times as large as the minimum fatal. With the minimum fatal dose of physostigma, it was found that while ·01 grain of atropia is too small to prevent death, ·015 grain is able to do so; and that with any dose ranging from ·015 grain to 5·2 grains the lethal effect of this dose of physostigma may be prevented; while if the dose of atropia be 5·3 grains or more, the region of successful antagonism is left, and death occurs. With one and a-half times the minimum fatal dose of physostigma, successful antagonism was produced with doses of atropia ranging from ·02 grain to 4·2 grains; with twice the minimum fatal of physostigma, with doses of atropia ranging from ·025 grain to 3·2 grains; with two and a-half times the minimum fatal of physostigma, with doses of atropia ranging from ·035 grains to 2·2 grains; with thrice the minimum fatal of physostigma, with doses of atropia ranging from ·06 grain



ANTAGONISM BETWEEN PHYSOSTIGMA A
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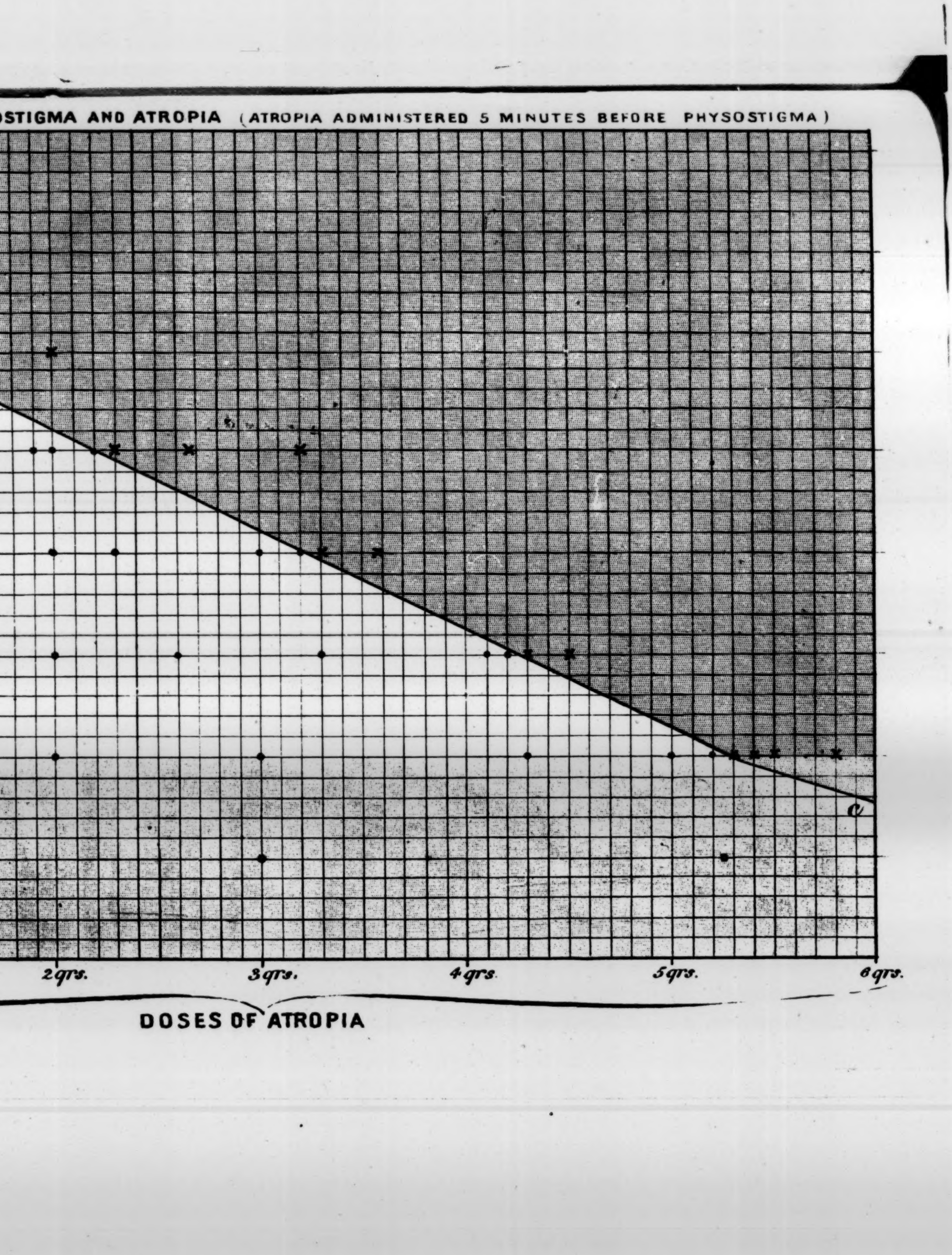
STIGMA AND ATROPIA (ATROPIA ADMINISTERED 5 MINUTES BEFORE PHYSOSTIGMA)
C
2 grs.
3 grs.
4 grs.
5 grs.
6 grs.
DOSES OF ATROPIA

to 1·2 grain; and with three and a-half times the minimum fatal dose of physostigma, with doses of atropia ranging from ·1 grain to ·2 grain. Successful antagonism could not be obtained above this dose, and, accordingly, three and a-half times the minimum fatal dose of physostigma would appear to be about the largest quantity whose lethal action may be prevented by administering atropia five minutes previously.

A similar series of experiments has been made, in which physostigma was administered five minutes before atropia, and the results were essentially the same, excepting that the region of successful antagonism was found to be more limited.

These results may be graphically represented by means of diagrams. The diagram accompanying this abstract is a reduced copy of one exhibited by the author to illustrate the series of experiments above described, in which atropia was administered five minutes before physostigma. The experiments that terminated in death are marked by crosses, and those that terminated in recovery by dots, while the position assigned to each experiment is determined by the doses of physostigma and atropia, calculated, when necessary, for three pounds weight of rabbit. The doses of atropia increase according to the distance, in a horizontal direction, from the perpendicular line forming the left margin of the diagram, and the increase proceeds at the rate of one-tenth of a grain for each subdivision of the horizontal lines. The doses of physostigma increase from below upwards, the same horizontal line always representing the same dose of physostigma. The curved line, *a b c*, separates the fatal experiments (crosses) from those which terminated in recovery (dots), and, accordingly, it defines the region of successful antagonism—a region further distinguished in the diagram by the absence of shading. The *darkly* shaded region is that in which antagonism is not successful, death being produced because the doses of atropia given in combination with one or other of the doses of physostigma employed are either too small or too large. In the *lightly* shaded region, below the horizontal line representing the minimum fatal dose of physostigma, the doses of physostigma are too small of themselves to cause death. The lateral extension of the diagram is, however, insufficient to exhibit the chief interest of this region. Were the diagram extended, it

would show that fatal experiments occur in this region, not only with fatal doses of atropia given in combination with less than fatal doses of physostigma, but also with less than fatal doses of atropia given in combination with less than fatal doses of physostigma.

In this manner, the entire *superficial area* of the region of successful antagonism has been defined, when physostigma is given five minutes after and five minutes before atropia. In addition to this, what may be termed the *thickness* of the region has been determined. For this purpose, series of experiments were made, in each of which the doses of physostigma were the same, and the doses of atropia varied; while with each dose of atropia, several experiments were made which differed from each other by a difference in the interval of time between the administration of the two substances. From the data thus obtained, curves have been constructed; the dose of physostigma serving as the base-line, the various doses of atropia as the abscissæ, and the different intervals of time that separate successful from unsuccessful experiments as the summits of the ordinates. When these curves are brought into relation with a diagram of the superficial area of the region of successful antagonism, in such a manner that the base-lines, representing the doses of physostigma, correspond to each other, and that the ordinates of these curves extend at right angles to those in the diagram of the superficial area, the lateral extension of the region of successful antagonism may be defined. In this way, its lateral as well as its superficial extent has been indicated with atropia and physostigma.

After defining the superficial area and the thickness of the region of successful antagonism, it seemed of interest to ascertain what dose of atropia is required to produce death with a dose of physostigma below the minimum fatal. The experiments performed for this purpose show that when one-half of the minimum fatal dose of physostigma is given five minutes after atropia, so large a dose of the latter substance as 9·8 grains is required in order to cause death; recovery taking place with doses ranging from 3 to 9·5 grains.

The minimum fatal dose of sulphate of atropia given alone was found to be twenty-one grains for a rabbit weighing three pounds.

It is, therefore, remarkable that the $\frac{3}{200}$ths of a grain can prevent a dose of physostigma, equal to the minimum fatal, from causing death, and that the $\frac{1}{10}$th of a grain is capable of rendering non-fatal a dose of physostigma, equal to three and a-half times the minimum fatal.

Excepting dilatation of the pupils, these minute doses of atropia, and indeed any dose capable of antagonising the lethal action of physostigma, are unable to produce any symptom recognisable by a mere inspection of the animal. Still, they undoubtedly produce energetic physiological effects—effects, however, which it is unnecessary to describe in this brief abstract. It is sufficient to point out that the notion, which exists in many quarters, that rabbits can scarcely be affected by atropia is an erroneous one.

Without referring to the other results obtained in his investigation, the author pointed out, in conclusion, that unless the antagonism between any two active substances be examined in the manner indicated in this communication, no satisfactory proof of its existence can be obtained. The superficial area of the region should always be defined, otherwise indications of antagonism obtained by one observer will be liable to be discredited by those who subsequently examine the subject. The first observer may succeed in performing an experiment within the area of successful antagonism, and thus feel satisfied of its existence; but his successors may fail in obtaining any proof by so varying the dose of one or other substance as to pass the limits of the region of success (see diagram). Feeling assured that many examples of successful antagonism, besides the one he had the honour of bringing before the Society, will yet be discovered, the author could not avoid the conclusion that the imperfect methods of investigation hitherto pursued are accountable for the absence of success that has attended the numerous researches made on this subject—a subject, it need scarcely be added, of the greatest importance to toxicology and to scientific therapeutics.

6. On the Homological Relations of the Cœlenterata. By Professor Allman, F.R.S.E.

Abstract.

In this communication an Actinozoon (*Actinia*) was compared with a Hydrozoon (*Hydra*), and the various Sub-orders of the *Hydrozoa* were compared with one another.

The author agreed with Agassiz in regarding the radiating chambers of an *Actinia* as the homologues of the radiating canals of a medusa, but he differed from him as to the true homologies of the differentiated stomach-sac of *Actinia ;* for while Agassiz regards this as represented by the proboscis or hypostome of the *Hydra* inverted into its body cavity, Professor Allman maintains that it is impossible on this supposition to conceive of the structure of *Actinia ;* and on comparing a *Hydra* with an *Actinia*, he imagines the tentacle to become connate for a greater or less extent with the sides of the hypostome and with one another, so that the hypostome of the hydra, while retaining its normal position, will thus become the stomach of the Actinia, and will at the same time become connected with the outer walls by a series of radiating lamellæ—the connate tentacle walls—separated from one another by radiating chambers, the cavities of the tentacles; while such portions of the tentacles of *Hydra* as still continue free will be represented by a single circle of the tentacles of *Actinia*.

The author had formerly compared the radiating canals of a hydroid medusa to the immersed portions of the tentacles of a *Hydra*, and he still maintains this view.

The strict parallelism of a siphonophore with a hydroid was pointed out, and each of the zooids which combine to form the heteromorphic siphonophorous colony was shown—as indeed Huxley and others had already done—to have its representative in the hydroid colony, and to be but a slightly modified form of some hydral zooid.

In order to understand the relations of a discophorous or steganophthalmic medusa to the other *hydrozoa*, he supposes the "atrium" of a hydroid medusa, or that part of the main body cavity which is still immersed in the solid proximal portion of the

umbella, at the base of the manubrium, to be expanded laterally, and the gelatinous extoderm of its floor to be projected along four or eight symmetrically disposed radiating lines into as many thick pillars, which converge towards the axis, and there meet the manubrium, while the thin intervening portions between the pillars become developed into generative pouches, the velum at the same time disappearing. A hydroid medusa would thus, in all essential points, become converted into a discophorous medusa.

A *Lucernaria* was conceived of by imagining a *Hydra* to have its tentacles reduced to four in number, and expanded laterally until their sides meet and coalesce; while the hypostome continues free, the solid hydrorhizal basis becoming at the same time extended into a peduncle of attachment traversed longitudinally by four canal-like prolongations of the body cavity, or else by a simple continuation of this cavity.

Lastly, a *Beroe* was taken as a type of the *Ctenophora*, and was conceived of as a hydroid medusa so modified as to become reduced to the atrial region alone. The two lateral canals which spring from the somatic cavity in *Beroe*, and subdivide so as to form ultimately the eight meridional canals, correspond to the greatly developed basal portion of the radiating canals of the medusa, or that portion of those canals which is still contained within the solid summit of the umbella; the affinities of the *Ctenophora* being thus directly with the *Hydrozoa* instead of the *Actinozoa*.

The author finds the key to the homology of *Beroe*, and the transition between the *Ctenophora* and the *Hydroida* in the singular ambulatory gonophore of *Clavatella*.

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